THURSDAY, MARCH 14, 1895.

THE LIFE OF DEAN BUCKLAND.

The Life and Correspondence of William Buckland, D.D., F.R.S. By his Daughter, Mrs. Gordon. (London: John Murray, 1894.)

IN the early decades of this century Geology had not established for itself an acknowledged place in the circle of the natural sciences. With as yet no settled philosophical basis, it offered a boundless field for indulgence in the wildest conjectures and the boldest speculation. Its votaries were, therefore, hardly regarded as serious students by the scientific men of the day. On the other hand, they incurred much popular odium. They had drawn such strange and almost incredible pictures of what they averred to have been the past history of the earth, so utterly at variance with all accepted beliefs, that they were looked upon with suspicion by some, sneered at by others, while by a large body of blatant opponents they were openly denounced as freethinkers, who, under the guise of natural science, aimed at the subversion of all religion. It needed some courage to be a geologist in those days, and still more to be a champion of the new inquiry.

Among the most prominent and effective of those who stood in the front of the battle and fought the hard fight was William Buckland. No man did more than he to raise geology from its depressed beginnings to the dignity of a definite branch of natural knowledge. And his name will ever be remembered with gratitude and affection as that of one of the clear-eyed, large-hearted fathers of English geology. This proud position he gained by a two-fold claim. The amount, the wide range and the intrinsic value of his original contributions to the science would have been enough to place him in the front rank of the leaders in that heroic age of geological discovery. But it was not merely, perhaps not even chiefly, by these qualifications, admirable as they were, that he attained to his unquestioned pre-eminence among his contemporaries. It was rather the unique personality of the man which gave him his remarkable influence, irresistibly engaging the sympathies and admiration of all who came in contact with him, bearing down gently but firmly all opposition, and gaining for him the affectionate esteem even of those from whom he differed.

His solid work remains and can be examined and weighed, and its influence on the progress of his favourite science can be accurately determined. But that personal sway passed away with him who wielded it, and is now only a memory. It has not yet been adequately pictured for the comprehension of those who never felt it, or who coming after have only heard feeble narratives of it, together, perhaps, with some of the many quaint stories still in circulation that illustrate it. At the time of his death numerous appreciative obituary notices of him appeared, some of them by personal friends who knew him well, and mourned the loss of so much that was bright, inspiriting and lovable. Some pleasant recollections of him by his son Frank were published in the last edition of Buckland's famous Bridgewater Treatise. But some more detailed biography might have been expected. He was a ready and copious correspondent, and it might have been supposed that abundant material must remain to furnish a picture of the man himself in his daily life, in his family, in his lecture-room, in his warm discussions with his friends, in his rambles with his students, in his intercourse with farmers and masons and labourers, and in his correspondence with many of the most interesting men of his time.

It was therefore with no little expectation that we received the volume of which the title is placed at the head of this notice. We have read it with interest and pleasure, and yet, it must be frankly confessed, with some measure of disappointment. Mrs. Gordon deserves the thanks of all lovers of science for the filial devotion and affectionate enthusiasm with which she has prepared this memoir of her father. We rise, indeed, from the perusal of the book with a somewhat fuller knowledge of Buckland's career, and with a little more insight into that personality which gave him his charm and his influence. But the information is neither of the kind nor of the amount which might have been looked for from the title of the volume.

Nearly half a century has passed since Buckland was stricken down by the malady which removed him from active life, and after some eight years of sad seclusion carried him to his grave. There can be few alive now who remember him in his prime, and from whom reminiscences of the man could be obtained. Mrs. Gordon appears to have done her best to procure such records from surviving friends, and she has received several of much interest. But it was from his own letters and jottings that most of real importance was to be looked for. The "correspondence," however, which appears on the title-page, forms but an insignificant part of the book. Long extracts are given from his lectures, his addresses and his sermons. Those who want to know more of the man himself would gladly exchange these citations for the brief notes and the longer epistles, in which he continually unbosomed himself and told so graphically what he was doing and thinking about from day to day. That Mrs. Gordon has made such comparatively slight use of such material must mean, we fear, that she has not been able to recover it. In that case we must sympathise with her disappointment. The task, perhaps, has been too long delayed.

In looking over the nature and amount of the work done by Buckland during his busy life, one is astonished at the great extent of subjects which claimed his attention, and in which he laboured and wrote. At one time he is absorbed in the study of changes of topography, whether it be the valleys of the south of England, or the solution of the chalk, or the destruction of the coast by landslips, or the sculpturing of the Highlands and of Wales by glaciers. At other times, or even when he had some of these topographical questions in hand, we find him hard at work upon problems in tectonic geology —British or foreign—the structure of the Alps, the geology of Nice, our south-western coal-fields, or the coast of Dorset. But undoubtedly it was the palæonto-

logical side of geology that most fascinated him. And what a mass of observations he accumulated in that department of the science! Every grade of the animal kingdom had an interest for him. He was passionately devoted to living animals, and he made use of his knowledge of them and their ways in interpreting the remains of their remote ancestors imbedded in the geological formations. He would take endless trouble to satisfy himself as to the habits of some living animal, in the hope of thereby throwing light on the history of extinct forms. Witness, for example, his rapid journey to the Pentland Hills in Midlothian, for the purpose of examining the drained bed of a large reservoir, where he expected to find materials for elucidating the history of old lacustrine limestones.

In those days geology had not become a science of detail. There were new fields to be cultivated on every side, and Buckland was the first to enter some of these. His researches in caves opened up a fresh chapter in geological history. And his chivalrous support of Agassiz, in the face of much ridicule, when he announced the former existence of glaciers in Britain, must be recognised by all glacialists as one of the first steps which led to the recognition and cultivation of glacial geology in this country.

There was ever in Buckland's science a strong vein of practical common sense. He was imaginative beyond most of his compeers, and sometimes, perhaps, allowed his imagination too free a rein, but he never lost sight of the fact that geology has a very definite practical side, and may be turned to useful account in many of the affairs of daily life. He was an active farmer, in order that he might try various methods for the improvement of crops. To him we owe the introduction of coprolites, so valuable a source of artificial manures. He never lost an opportunity of preaching the true principles of drainage, and he insisted on the value of geological knowledge in all questions of water-supply. These are familiar enough applications of the science now, but it was largely through Buckland's influence that they were recognised.

Of the man himself as he lived and moved, Mrs. Gordon's volume gives a pleasing though hardly adequate picture. His boundless energy and enthusiasm were infectious, and led many a man and woman captive into the geological fold. His industry enabled him to carry on a busy scientific life, while at the same time he had on hand enough of other work to fill up fully the time of most men. His wide sympathies and large range of knowledge broadened his grasp of his own special science, and led him to see where he could find the most useful collateral information. His eloquence as a speaker and writer commanded the attention of his audiences, and did much to make his subject popular. His unwearied hospitality and his generous large-handedness opened a way for him into the hearts of men, while his overflowing vivacity, his brilliant wit, and his racy talk made him the central figure in any company where he might happen to be. Truly there were giants in the land in those days, and no one of them deserves to be more warmly remembered for all that he did, and all that he was, than William Buckland.

OUR BOOK SHELF.

The Birds of Eastern Pennsylvania and New Jersey. Edited by Witmer Stone. Pp. 185. (Philadelphia: Delaware Valley Ornithological Club, 1894.)

THE Delaware Valley Ornithological Club has only been established about five years, but steps were taken shortly after its organisation to compile a list of the birds observed by the members in the vicinity of Philadelphia. In this volume the important results of the club's ornithological investigations are brought together in a compact form by the committee of three—Messrs. Morris, Rhoads, and Stone—appointed to prepare the work. A list of the birds to be found in the Delaware Valley and along the New Jersey sea-coast has naturally a limited sphere of usefulness, even though it may furnish a work of reference for ornithologists in general. But this volume contains not only an annotated list of the birds of the district to which it refers, and a bibliography of ornithological literature relating to Pennsylvania and New Jersey; it comprises, in addition, outlines of the knowledge of the geographical distribution and migration of birds, thus giving it increased value. These chapters

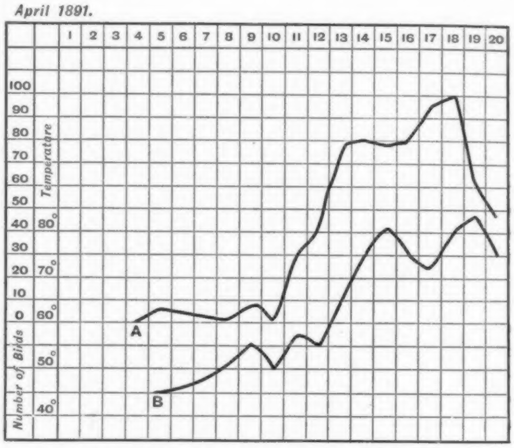

FIG. 1.—A, Migration Curve; B, Temperature Curve.

will lead beginners in the study of birds to understand the importance of simple observations.

The influence of meteorological conditions upon the migration of birds is an important point, and one which requires careful investigation. The records of the club's observers furnish some valuable facts for the study of migratory waves or rushes, and their relation to meteorology. It is pointed out that, during migrations, the flight of birds is not uniform, but is made up of a series of waves or rushes and rests or lulls. The relation of these rushes to temperature is well shown in the accompanying diagram, reproduced from the volume. The curve A represents graphically the fluctuation of the April migration of 1891, based upon observations of the Flicker, Chipping Sparrow, and Brown Thrasher; the curve B shows the temperature variation during the month of observation, based upon the daily maximum temperature. The connection between the two curves is very distinct, and it is especially interesting to observe that the "bird waves" occurred a day or two after a decided increase of temperature.

In conclusion, we think the Delaware Valley Ornithological Club is to be congratulated upon its activity, and Mr. Stone for this admirable addition to the literature on the birds of Eastern Pennsylvania.

Conspectus Floræ Africæ, ou Enumération des Plantes d'Afrique. Par Th. Durand et Hans Schinz. Vol. v. Monocotyledoneæ et Gymnospermeæ. 8vo. Pp. 977. (Bruxelles, 1895.)

IT may, perhaps, be asked why the fifth volume of a work should appear before the fourth and all the preceding ones. Doubtless the authors were influenced thereto by the fact that neither Oliver's "Flora of Tropical Africa," nor Harvey's "Flora Capensis," has reached the groups enumerated in the bulky volume under notice. Certainly this course has the advantage of utility, and will be of great service in the elaboration of the continuation of the works named. As an index to the scattered literature of the subject, the present volume is indeed invaluable. It covers all that may be called African, including the Atlantic islands from Madeira to Tristan d'Acunha, and the islands of the Indian Ocean, from St. Paul and Amsterdam to Mauritius, Madagascar, and Socotra. It is true, the geography of the plants is not worked out all through so fully as Mr. C. B. Clarke has done the Cyperaceæ. For instance, the characteristic grass of Tristan d'Acunha and St. Paul and Amsterdam islands, *Spartina arundinacea*, is only recorded from the former group. In other respects, Mr. Clarke's elaboration of the 800 Cyperaceæ is by far the most complete and thorough part of the volume, though it is blemished by the introduction of a very large number of names of new species without descriptions.

But, leaving all criticism out, this volume will be welcomed alike by horticulturists and botanists; by the former, more especially, because it contains the petaloid monocots, so numerous in South Africa. Synonyms and references to figures in the various illustrated serials add to the usefulness of the enumeration. To give an idea of the extent of this compilation, it may be mentioned that the Liliaceæ include nearly 1100 species, belonging to 67 genera. *Aloe* alone numbers nearly 100 species. The Irideæ are about 700 strong; *Gladiolus* being the largest genus, with 143 species. Orchids also exceed 1000 species, belonging to 74 genera; and 160 species of *Habenaria* are enumerated. Palms are less numerous than might have been expected, considering the comparatively large number in a small group of islands like the Seychelles. Only 63 species are given, which is about a quarter the number inhabiting British India. This is largely due to the genus *Calamus* being represented by only one species in Africa, whereas there are 72 in India.

W. B. H.

Leçons de Chimie. Par H. Gautier et G. Charpy. (Paris: Gauthier-Villars, 1894.)

THE general plan of the second edition of this work does not differ essentially from that of its predecessor. The introductory portion on generalities—dealing with states of aggregation, laws of combination, equivalents and atomic weights, physical and chemical transformation, chemical equilibrium, the velocity of reaction, thermochemistry, &c.—has been recast, and now occupies one-fifth of the volume. In the descriptive portion, which is concerned with inorganic chemistry only, Moissan's work on fluorine, the diamond, and boron has been introduced. It is characteristic of a French text-book that even now it is deemed necessary to print alongside each important atomic equation the corresponding equation based on equivalents. In the same connection it will be somewhat disconcerting to English students to find that "Le poids atomique est égal au poids équivalent pour les éléments suivants: . . . Pour tous les autres éléments, la valeur du poids atomique est double de celle de l'equivalent." In other respects the book is well up to date, and contains much useful information expressed with the clearness and precision for which French text-books are deservedly famed.

J. W. R.

LETTERS TO THE EDITOR.

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Variation and Specific Stability.

I AM afraid that in my anxiety to compress too long a statement, I did not make the points which I wished to bring forward in the recent discussion at the Royal Society sufficiently clear. I have therefore written out the following summary:—

(1) All organisms vary. That in doing so they obey Quetelet's law was suggested by Mr. Darwin himself more than twenty years ago. He observes (NATURE, September 25, 1873, p. 432):—"It is known from the researches of Quetelet . . . that men may be grouped symmetrically about the average with reference to their height. . . . We may presume that this is the usual law of variation in all the parts of every species under ordinary conditions of life."

(2) Prof. George Darwin supplemented this in a following number (October 16, p. 505) with a very lucid account of the principle. In this he says:—"We may assume with some confidence that under normal conditions, the variation of any organ in the same species may be symmetrically grouped about a centre of greatest density."

(3) A well-known illustration is that of a marksman shooting at a target. The distribution of his shots will follow the same law; they will be grouped round a centre of greatest density, which is easily ascertained, as it is the centre of gravity of the circumscribed figure. And on successive trials, if all conditions remain unaltered, the position of the centre will remain the same, though the positions of the shots will be different.

(4) No two individual representatives of a species in nature are exactly alike. All differ in some respect. We may picture the aggregate, however, as grouped with respect to any discriminating character like the shots on the target. Our conception of the species to which they belong is an abstraction which we endeavour to represent in our museums by a specimen which would be placed as near as possible to the centre of greatest density. Such an abstraction we may call the *mean specific form*.

(5) Returning to the case of the target, it is obvious that if some new condition be introduced, such as a wind blowing transversely, every shot will be affected, and the centre of density of the system will be shifted. What is the analogous result when we are dealing with the aggregate of individual organisms representing "a species"?

Natural selection will come into play, to begin with. It may be that some hitherto indifferent variation may be favoured by the new condition. Others will be relatively handicapped, and such a favoured variation will get the upper hand. It is obvious that the result will be to shift the centre of density: the mean specific form will have undergone a corresponding change.

(6) It is probable that so simple a result is not the usual one, and what actually takes place is much more complex.

Mr. Darwin concludes "that organic beings when subjected during several generations to any change whatever in their conditions, tend to vary." ("Variation of Animals and Plants," ii. p. 250.) I infer therefore, and all the facts which have come under my observation confirm it, that a change in the external conditions, otherwise the *environment*, will provoke *some* variation in the organism, which I may call the *stimulated variation*.

(7) It appears to me that from the Lamarckian point of view, the stimulated variation ought to be immediately adaptive. From the Darwinian this is not necessarily the case. It may be either advantageous or, at any rate, indifferent. ("Changed conditions generally induce mere fluctuating variability." Darwin, "Origin," 6th ed. p. 131.) Prof. George Darwin, in the note above cited, traces out the result in the two cases. In the former case, "with continual intercrossing," the new variation will get the upper hand, and the centre of density will be shifted; in the latter it will, by continuous "weeding out," be, after a temporary displacement, eventually restored.

(8) This leads to the consideration of the *stability* of the mean specific form. Some species seem to yield pretty rapidly, though with an appreciable *inertia*, to the influence of changed condi-

tions; others seem almost indefinitely to resist it. Probably, however, even the most intractable cases may eventually be broken down. This stability has been largely used as an adverse argument to organic evolution generally. Prof. Decaisne was much influenced by it, and stoutly maintained not merely the permanent stability of species but even of varieties. His experiments on the seminal reproduction of cultivated pears, which extended necessarily over a long series of years, was, as might be expected, adverse to varietal stability, and left him in a position from which he never extricated himself.

The existence of specific stability is, however, undoubted. Mr. Carruthers devoted his address to Section D of the British Association in 1886 to a useful summary of the ascertained facts on the subject. He laid particular stress on the well-known data, which are illustrated in the Kew Museums, as to the flora of Egypt. This, 4000 years ago, was composed of species which differed apparently in no particular from their present living representatives.

An even more striking illustration is afforded by the history of standards of weight. From Prof. Ridgway's researches it appears that these were originally based on seeds ("Origin of Currency and Weight Standard"). He finds (p. 182) that "the Troy grain is nothing more than the barley-corn." Further, "in 1280 (8 Edward I.) the penny was to weigh 24 grains, which . . . were as much as . . . 32 grains of wheat" (p. 180). The ratio still obtains. "In September, 1887, I placed in the opposite scales of a balance 32 grains of wheat 'dry and taken from the midst of the ear,' and 24 grains of barley taken from ricks of ears grown in the same field at Fen Ditton, near Cambridge, and I thrice repeated the experiment; each time they balanced so evenly that a half-grain weight turned the scale." Further, he found that "practically 4 wheat grains = 3 Troy grains" (p. 182). The same fact of stability can be illustrated from other seeds used as standards of weight. No working naturalist will then be disposed to very much quarrel with the conclusion arrived at by Mr. Carruthers, that "species must be dealt with as fixed quantities."

(9) Notwithstanding this, the counterfact remains that it is doubtful if there is any species the stability of which cannot be broken down by appropriate methods. I, however, prefer to say not that a species is "fixed," but that its mean specific form is appreciably stable under permanently uniform conditions. And this is probably what Mr. Carruthers really meant.

The interesting question then arises, how is this stability brought about?

(10) The principle of stability seems to me scarcely to have received the attention it deserves. Mr. Darwin has, I think, never expressly stated it, though he was evidently well acquainted with it, and has supplied some explanation of its causes.

In dealing with the facts referred to above as to the Egyptian flora, he simply says: "In Egypt, during the last several thousand years, the conditions of life, as far as we know, have remained absolutely uniform." ("Origin," 6th ed. p. 169.) The implication, though he does not actually say so, is that under such circumstances evolution is in abeyance. If it were not, it seems to me that we must admit Nägeli's "innate tendency towards progressive and more perfect development." Mr. Darwin states, however, the correlative principle with greater definiteness. "The influence of changed conditions accumulates, so that no effect is produced on a species until it has been exposed during several generations to continued cultivation or domestication." ("Animals and Plants under Domestication," ii. 261.) I have no doubt that this is true, and I entirely agree with De Vilmorin, cited by Mr. Darwin (*l.c.* p. 262) that "the first step is to get the plant to vary in any manner whatever, . . . for *the fixed character of the species* being once broken, the desired variation will sooner or later appear."

More or less close interbreeding will, it is well known, tend to stability. Mr. Darwin tells us: "After a dozen generations of self-fertilisation, it is probable that the new variety would remain constant, even if grown under somewhat different conditions." ("Cross and Self-fertilisation," p. 460.) The same fact is illustrated "by the survival during at least half a century of the same varieties of the common pea and the sweet-pea." (Darwin, *l.c.* p. 39.) Self-fertilisation has the positive advantage that it "assures the production of a large supply of seeds." (Darwin, *l.c.* p. 41.) The price ultimately paid is, however, probably a very heavy one—that of extinction. (*Cf.* Darwin, "Life," iii. 276.)

(11) The case of wheat is peculiarly interesting. It is not worth while pressing the evidence too far. But it may be remarked that wheat has by no means lost its power of variability. Mr. Carruthers remarks:—"The improved varieties of our cereals now under general cultivation have been obtained almost entirely from the selection of individual plants." (*Journ. R. Agric. Soc.* 4th ser., iv. p. 684.) Mr. Darwin ("Animals and Plants," i. 318) quotes the authorities for the statement that the wheat cultivated by the lake-dwellers in Switzerland had very small grains. Still we may admit that for a very long period the average wheat grain has not appreciably varied. This appears to me to prove nothing more than that wheat has got into a stable state, which I should attribute to the average uniformity of the conditions under which it has been cultivated. I shall not press the question of its self-fertilisation, which is generally accepted, but which Darwin (*l.c.* i. 316) thinks not invariable. Somewhere he has stated, though I cannot put my hand on the reference, that the size of the grain is disadvantageous.

If so, this would be unfavourable to its increase. De Candolle ("L'origine des plantes cultivées," p. 370) regards the original source of the wheat-plant as extinct, and suggests that the attractiveness of its grains for birds may have been the cause. At any rate it appears certain that but for cultivation it would now entirely disappear. The conclusion seems to be that mankind has brought it up to a certain standard which was sufficient for its purpose, and has not been compelled to carry it further by unconscious selection.

The case of maize, which has also been quoted, is much less to the point. It is now pretty certain that its origin is to be found in a form still existing in which the grains are only two-rowed. But maize, under existing cultivation, proves to be extremely variable.

(12) It appears to me that an important light is thrown upon what I may be allowed to term the *stability problem* by the remarkable investigation recently presented to the Royal Society by Prof. Weldon. It is one which I am persuaded would have given Mr. Darwin peculiar pleasure.

Prof. Weldon measures very carefully some organ in each individual of a given population. The measurements are plotted out along a base line; the number of individuals in which each measurement occurs are represented proportionately by lines drawn perpendicular to the base line. The summits when connected form a curve, which is termed a *frequency curve*. For a discussion of the general principle and of the interpretation of such curves reference must be made to the able investigation for which biologists are indebted to Prof. Karl Pearson (*Phil. Trans.* 1894, A., pp. 71–110).

Prof. Karl Pearson (p. 72) terms "a frequency curve which for practical purposes can be represented by the error curve," a normal curve. Such a normal curve, I apprehend, is only another way of representing the result of Quetelet's law, which, as I stated above, there was reason to suppose was the ordinary law of variation. For he adds: "When a series of measurements gives rise to a normal curve, we may probably assume something like a stable condition; there is production and destruction impartially round the mean.

By applying the method to a crab from Plymouth Sound, Prof. Weldon has been able to throw light, it seems to me for the first time, on a most important factor in the stability problem.

"About 7000 females . . . were chosen (at random, except as regards their size), and two dimensions were measured in each. The results were then compared with those of the corresponding measurements, made upon a sample of 1000 adult females from the same locality."

The result was to show that "selective destruction" takes place in early life amongst individuals, which deviate from the "mean specific form." Prof. Weldon arrives at the conclusion "that the position of minimum destruction should be sensibly coincident with the mean of the whole system," is a condition which "may be expected to hold for a large number of species, which are sensibly in equilibrium with their present surroundings, so that their mean character is sensibly the best." The actual statistical demonstration of this fact, in my opinion, deserves to rank amongst the most remarkable achievements in connection with the theory of evolution.

(13) When the frequency-curve is abnormal, Prof. Karl Pearson infers "the pressure at a given time of some particular form of natural selection." The mathematician may in this case be able

to render assistance to the biologist of supreme importance. "The amount and direction of the abnormality will be indicated if this [abnormal] frequency-curve can be split up into normal curves." Analysis would in this way give us information which we could perhaps not even guess at.

I agree with Prof. Karl Pearson that "resolution into two" will be sufficient. The stress of natural selection at any moment must always be between the best and the next best.

In tolerably simple cases I have no doubt that the result of Prof. Karl Pearson's labours will be to throw great light on the matter. In more complicated ones we must look for some disappointment in view of "the great variety of solutions which may be suggested" (p. 106). And the tentative discovery of "component normal curves" seems likely to be fallacious (p. 90).

(14) I think it is important to insist that the importance of Prof. Weldon's present results has reference to the stability problem. He is fully aware of this fact when he says that "they cannot be expected to hold in cases of rapid change such as those induced artificially by selection under domestication, or naturally by rapid migration or other phenomena resulting in a rapid change of environment." These will lead to abnormal frequency curves.

(15) A few remaining points in Prof. Weldon's paper deserve some remarks.

I entirely agree with him in minimising the value of "sports" in evolution. As against Nägeli and his followers, I see no ground for believing in any innate progressive tendency in organisms. When the organism is in stable relation with its environment it will continue so indefinitely. That is the conclusion I deduce from the flora of Egypt, and other facts which have been cited of the same kind. Prof. Weldon seems to me to have supplied this position with a most important proof by establishing the "selective destruction" of variations aberrant from the mean specific form. When the environment varies, stability is destroyed; but it will be ultimately re-established, though with a different centre, by the operation of natural selection. The result is that the organism has undergone some permanent degree of change. As I conceive the process, it is one of continuous adjustment of "slight" variations on one side and the other. But it is important to keep in view that variation in the environment stimulates the variation in the organism which supplies the ultimate material for adjustment. That the amount of the adjustment at any moment is slight is not incompatible with its amounting to almost anything we like in the aggregate, if sufficient time be allowed. We might as well deny that a curve can be built up from its infinitesimal elements.

The value in this respect of sports may be easily overrated. It appears to me that generally, so to speak, they attempt too much and overshoot the mark. The improbability of a casual sport being exactly what is wanted to bring the organism into an advantageous relation to the environment at any particular juncture seems to me very great. That such a thing may occur is not denied, but it can hardly be more than a "fluke."

(16) This is confirmed by the fact that in the vegetable kingdom sports are rare, and they seem to have little power of holding their own in competition. I instanced the cases of the occurrence of copper-coloured and laciniate foliage in many trees, as well as the occurrence of varieties with weeping and fastigiate habit. It is well known that to some extent these are perpetuated by seed. But the existence of such forms would undoubtedly be transient if it were not for their perpetuation as curiosities under cultivation.

(17) In museums it is usual to attempt the representation of the mean specific form. The association of strikingly aberrant specimens is interesting and often suggestive. But I do not see that they illustrate more than the possibilities of variation and the fact that it may be discontinuous.

(18) I placed upon the table plants of the feral type and of a recent cultivated form of *Cineraria cruenta* from the Canaries. The difference in habit and in the form and colour of the flowers was enormous. This has undoubtedly been brought about by human selection. As far as is known it has been accomplished by the gradual accumulation of small variations. The horticulturist has not troubled himself about the foliage, which, though more luxuriant, has remained practically unchanged. But it must not be assumed that it is unchangeable. In the case of the Chinese primrose, the feral form from north-west China, for which we are indebted to Dr. Henry, has palmatifid leaves rounded in outline; but the distal lobes are occasionally lengthened out. The horticulturist, as a matter of fancy, working on this, has now split the type into two races, one of which has palmatifid and the other pinnatifid leaves. Many botanists would, undoubtedly, if they did not know their history, assign to such a different specific rank.

(19) I am not sure that I quite understand Prof. Weldon when he says that "the statistical method is the only one at present obvious by which [the Darwinian] hypothesis can be experimentally checked." In the first place, I should myself hardly call it experimental at all. In the next place, though I think it will throw important light on the stability problem, in the important cases where evolution is actually taking place, the mathematical analysis appears to me to be beset with very great difficulties. We must not, therefore, expect too much from it.

(20) On the other hand, museums, as at present organised, do not help very much the study of evolution. In the case of plants, I doubt if herbaria will ever be able to present material in a sufficiently compendious or complete form to be of much use. The study, however, of extensive series of a few species of insects ranging over the whole of a large geographical area, such as Mr. Elwes has brought together in the case of butterflies, must, it seems to me, afford most important material for future discussion. W. T. THISELTON-DYER.

Royal Gardens, Kew, March 10.

Do Plants Assimilate Argon?

IT is a well-known fact that some plants are able to assimilate nitrogen from the atmosphere and form compounds. Now, as argon cannot be induced—at least up to now—by any known process of inorganic chemistry or physical science to enter into a combination with one or more of the known elements, it occurred to me whether that peculiar power which produced the cell is not able to form combinations with argon. The experiment to grow suitable plants in an atmosphere of pure argon, or argon mixed with pure oxygen, on a bed of pure sand, &c., would easily settle the question.

If this experiment has not yet been made, perhaps you will find space in your paper for the above few lines.

Essen-Ruhr, March 6. E. BLASS.

THE first thing is obviously to find whether there is any argon in a nitrogenous vegetable; and experiments are now nearly completed in my laboratory to see if nitrogen obtained from peas contains any argon. Similar experiments are being made with nitrogen from mice. In a few days I shall know the results. But this is, of course, on the assumption that the process which liberates nitrogen also liberates argon; and it is by no means certain. It should be remembered that argon and nitrogen have absolutely no similarity, and that their occurrence together in air is a pure accident, due to the inertness of both.

March 10. W. RAMSAY.

The Measurement of Pressures in Guns.

IN a paper "On Methods that have been adopted for Measuring Pressures in the Bores of Guns" (Report of the British Association, 1894), Captain Sir A. Noble has remarked that it seems to him "that there is no method so satisfactory, despite its attendant labour, as that of *making the projectile write its own story*" (p. 540). That might be sufficient for *smooth* bore guns, where there was little or no friction in the bore, but it is quite unsatisfactory when applied to *rifled* guns, and especially B.L. guns.

The two methods of experimenting now in use employ the *pressure gauge* and the *chronograph*, both of which we will, for this occasion, suppose perfectly accurate. The pressure gauge measures directly the pressure of the powder gas P, the quantity wanted. The chronograph will measure P, the same pressure of the powder gas, *minus* F, the resistance offered to the motion of the shot by the rifling, the friction of the driving-band, &c., $= P - F$, where F is often very great. The difficulty is to see how these two different processes can confirm one another, as F is unknown and of great importance.

The only satisfactory method of determining the maximum pressure of powder gas at any point in the bore of a *rifled* gun is to measure it *directly*, by the pressure gauge, which requires many precautions to be taken, or by some other more simple method. F. BASHFORTH.

Horncastle, March 9.

The Velocity of the Argentine Earthquake Pulsations of October 27, 1894.

IN several recent notes in NATURE (pp. 232, 371, 393), attention has been drawn to the great Argentine earthquake of last October 27, and to the record of its pulsations in Europe. In one of these (p. 371) a rough estimate is given of the velocity, but a more detailed one seems desirable on account of the great distance traversed by the pulsations.

According to M. Noguès (*Comptes rendus*, vol. cxx. pp. 167–170), the epicentral tract includes Rioja, San Juan and Mendoza. There is thus some uncertainty as to the exact position of the spot from which the pulsations started. In the following estimate I have supposed it to coincide with San Juan.

San Juan is about 312 km. from Santiago, and 11,600 km. from Rome, the difference being 11,288 km. The earthquake was registered by a seismograph at Santiago at 8h. 50m. 26s. p.m., Greenwich mean time. The slight preliminary pulsations were recorded by the great seismometrograph at Rome at 9h. 7m. 35s., the first maximum at 9h. 49m. 50s., and the principal maximum at 9h. 55m. 40s. Assuming that the first maximum (or beginning of the larger pulsations) corresponds to the movement which started the seismograph at Santiago, it follows that the distance of 11,288 km. was traversed by the pulsations with an average velocity of 3·17 km. per second.

It should be remarked that this estimate agrees very closely with those obtained for the same phase of the movement in the cases of the Greek earthquake of April 27, and the Constantinople earthquake of July 10, 1894 (namely, 3·21 and 3·20 km. per sec. respectively).

For the first slight movements recorded at Rome, Charkow and Nicolaiew, we must admit either that the pulsations producing them started some time before the great earthquake, or else that they travelled with a far higher velocity. If they left San Juan simultaneously with the larger pulsations (*i.e.* at 8h. 48m. 48s.), their average velocity must have been 10·38 km. per second. The horizontal pendulums at Charkow and Nicolaiew also recorded these early movements, beginning at 9h. 8m. 36s. and 9h. 12m. 6s. respectively; soon after which the curves more or less completely disappeared. San Juan is about 13,625 km. from Charkow and 13,240 km. from Nicolaiew, the average velocities to these places being therefore 11·47 and 9·47 km. per second. The latter obviously corresponds to a later phase of the movement.

Whether the slight preliminary pulsations start before, or at the moment of, the earthquake, is a question of the greatest practical importance from the point of view of earthquake-warnings. To answer it, one of the Italian seismometrographs or a horizontal or bifilar pendulum should be placed beside a seismograph in the immediate neighbourhood of the centre of disturbance. C. DAVISON.

Birmingham, March 6.

The Society of Spelæology.

THE attention of your readers has already been called to the formation of this Society in Paris (NATURE, January 3), the promotion of which is due to the action and enthusiasm of M. E. A. Martel, the author of the beautifully-illustrated work "Les Abîmes," reviewed by Prof. Bonney in your pages of the 28th ult. This book describes and illustrates a number of extraordinary and often hazardous subterranean explorations in the underground caves and watercourses of the limestone districts of France, Belgium, Austria, and Greece. The Society is intended to carry on the work thus initiated by M. Martel and his devoted co workers in a more effective manner, and over a wider area than has been possible by private enterprise. The formation of the Society, M. Martel writes me, is now an accomplished fact. About 130 gentlemen of all nationalities, some of whom bear well-known names in the ranks of science, have signified their adhesion. A provisional code of rules has been printed and adopted, and a meeting has already taken place, under the presidency of the president-elect, M. F. Deloncle, Deputy for the Basses-Alpes.

The first article of the rules states the object of the Society as follows:—"The Society of Spelæology is instituted in order to ensure the exploration—to facilitate the general study—to cooperate in the regulation or ultilisation—of subterranean cavities of all sorts, known or unknown, whether natural or artificial; to encourage and aid with funds investigations relating thereto; in a word to popularise and develop in a way, at once practical and theoretical, utilitarian and scientific, researches of all kinds in the interior of the earth." The subscription for ordinary members is fifteen francs per annum. It is intended to publish a quarterly bulletin; to a copy of which each member will be entitled.

In order to fully carry out the objects of the Society, the programme of which is a comprehensive one, more members are required, and I shall be glad to furnish any of your readers who may wish to join the society with the proposal form or *Bulletin d'Adhésion*, or they may be obtained from M. Martel, General Secretary, 8 Rue Ménars, Paris.

M. Martel, I may say, is desirous of extending his investigations to the British Isles, if sufficient inducement be offered in the exploration of some large cave, as yet unworked or imperfectly known, and where his apparatus of rope ladders, collapsible boats, &c., would be useful aids. Information on this head will be thankfully received by me. MARK STIRRUP.

Bowdon, near Manchester, March 6.

Contraction of Trees caused by Cold.

THE splitting of forest trees by frost is often ascribed to the same cause which bursts a pipe charged with water when the temperature falls below 32° F., namely, the expansion of the water on turning into ice. Botanists know that this is not so, but the splitting is owing to a contraction of the wood by frost, similar, but in a less degree, to what happens when the wood is dried. With the thaw the trees expand to their original dimensions. Evidence of such contractions and expansions is furnished by the measurements herewith.

For some years past, I have regularly taken the girths of a number of forest trees during summer, in order to note the amount of growth. To do this accurately I have to use a steel tape, and of course to girth the trees at exactly the same place. My experience, thus acquired in measuring to a nicety, is a sufficient reason for confidence that the following figures are substantially correct.

Girth of trees in October 1894, when done growing and before the frost.			Girths, February 8, 1895, 9 a.m. Temp. 3° F.	Girths, March 2, 1895, 3 p.m. Temp. 39° F.	Amount of contraction with frost.
No.		Inch.	Inch.	Inch.	Inch.
1	Sycamore	$26\frac{3}{4}$	$26\frac{5}{8}$	$26\frac{3}{4}$	$\frac{2}{16}$
2	Sycamore	$22\frac{1}{2}$	$22\frac{3}{8}$	$22\frac{1}{2}$	$\frac{2}{16}$
3	Sycamore	33	$32\frac{13}{16}$	33	$\frac{3}{16}$
4	Elm	$28\frac{7}{16}$	$28\frac{1}{8}$	$28\frac{7}{16}$	$\frac{5}{16}$
5	Elm	22	$21\frac{3}{4}$	22	$\frac{4}{16}$
6	Elm	$19\frac{9}{16}$	$19\frac{3}{8}$	$19\frac{9}{16}$	$\frac{3}{16}$
7	Ash	$46\frac{3}{16}$	$45\frac{13}{16}$	$46\frac{3}{16}$	$\frac{6}{16}$
8	Oak	$42\frac{1}{2}$	$42\frac{3}{16}$	$42\frac{1}{2}$	$\frac{5}{16}$
9	Oak	$17\frac{9}{16}$	$17\frac{1}{4}$	$17\frac{9}{16}$	$\frac{5}{16}$
10	Oak	$35\frac{3}{16}$	$34\frac{13}{16}$	$35\frac{3}{16}$	$\frac{6}{16}$
11	Beech	$21\frac{1}{2}$	$21\frac{5}{16}$	$21\frac{1}{2}$	$\frac{3}{16}$
12	Beech	$32\frac{3}{4}$	$32\frac{7}{16}$	$32\frac{3}{4}$	$\frac{5}{16}$
13	Beech	$42\frac{1}{8}$	$41\frac{7}{8}$	$42\frac{1}{8}$	$\frac{4}{16}$

Bradford, March 4. J. CLAYTON.

The Barrenness of Precambrian Rocks.

REFERRING to the paragraph in NATURE (February 28, p. 423), on the sudden appearance of a rich fauna in the Lower Cambrian rocks, I should like to make a suggestion for the consideration of geologists. May not the extreme poverty of organic remains in Precambrian (Archæan) strata be largely due to a scarcity of carbonate of lime in the water of the Precambrian seas? The Uriconian and Longmyndian rocks of Shropshire, which, at the very least, must include five miles of sediment, comprise hardly a scrap of limestone. The same remark will apply to the Precambrian strata of Charnwood, South Wales, the mainland of North Wales, and the great Torridonian group of Scotland. The Pebidian rocks of Anglesey contain bands of limestone, it is true, but it is highly probable that they are of chemical origin, and not derived from oceanic waters. There are, of course, plenty of limestones in the older Archæan rocks of North America, and a few of them in the Lower Archæans of

Britain; but the proof of their marine origin remains to be written. They contain no undisputed organic remains. The rocks in which they are intercalated are not proved to be altered sedimentaries. There were numerous animals living in the Salopian area in the Longmyndian epoch, for their trails are quite abundant in some of the slaty seams; but, if there were no carbonate of lime in the sea, there could, of course, be little material to provide shells for its inhabitants. Numerous creatures of many types might have been evolved, whose soft tissue would leave no traces in the rocks. In succeeding ages, as the forces of denudation cleared off the newer Archæan, and cut down into partially decomposed crystallines, abundance of calcic carbonate would be carried down into the sea.

C. CALLAWAY.

Wellington, Shropshire, March 1.

The Artificial Spectrum Top.

IN your issue of March 7, we notice a letter from Dr. Dawson Turner, in which he says he has had a "Benham's Spectrum Top" made on glass, by an optician, for the lantern, and recommending others to do the same.

Will you allow us to state that we have sold all rights in this copyright top to Messrs. Pears, reserving to ourselves only the sole right of making them as lantern slides, in which form we have been supplying them for some time.

The tops can be obtained from Messrs. Pears, and the lantern slides from us; any one else supplying either will, of course, be infringing the copyright.

NEWTON AND CO.

RESEARCH IN EDUCATION.[1]

NO branch of *research work* at the present day offers greater opportunities, whilst none is more urgently in need of *original* workers, than that which lies open to the teacher in school or college; and it is surprising how small an amount of sound work is accomplished in it—how little it is realised that there is a science of education to be developed by persistent study, application and research. An analytical habit of mind seems to be the very last qualification sought for in a teacher—such is the influence acquired by clerical instructors in the course of centuries by the universal extension of methods of teaching originated in the monkish cell and cloisters, and wielded with but slightly diminished force, even to-day, by their lineal descendants, whose voices still preponderate in educational affairs. Conservative and sheep-like—as we cannot fail to be if all our early life be spent in an atmosphere of dogmatism—the slowness with which we evolve and apply new ideas is phenomenal. It cannot be that sterility is the outcome of excessive labour in days gone by, and consequent exhaustion of the soil; still less, that it is owing to absence of demand; for it is only too clear that the entire change in the conditions of life witnessed within the century renders it necessary that our children should be so educated that they may successfully grapple with the new conditions, and it stands to reason that the preparation which sufficed in the case of their forefathers must be insufficient in theirs. This is now being universally recognised, but all too slowly and imperfectly. Thus the academic oration first on my list, delivered only in September last, at Freiburg, by the Professor of Anatomy, is a vigorous protest against the practice prevailing in the German "Humanistic" Gymnasia of devoting an enormous proportion of the school-time to classical studies, and the consequent neglect of drawing, natural science, geography and modern languages, as well as of gymnastic exercises, which is very strange, as he points out, when it is remembered that the meaning of *gymnasium* is a place for athletic pursuits. He especially complains of the way in which exercises in classical style are insisted on and monopolise attention, and strenuously advocates their banishment from the three lowest classes at least. He refers with feeling to the pressure which is brought to bear in school and at home on the child to whom such work does not appeal, and the unhappy state of house and family on "style-days," remarking that every one who, like himself, has had this experience in his own person and that of his children, will sympathise with this view. He tells us that his own bitter experience of thirty-five years ago still follows him in his dreams, and that he can never forget the encouraging words hurled at him by the master of the "Prima" of the Stuttgart Gymnasium when he had done a bad Latin exercise—"You never in your life will come to any good, as sure as my name is Schmid." Is not this too often the attitude of our schoolmasters, and is it not too often forgotten that the human mind, fortunately, will not in all cases respond to one uniform system of treatment? Surely the time must soon come when it will be the main duty of headmaster and headmistress to study their scholars, and assort them in accordance with their aptitudes; when no headmaster will set down a boy as of inferior intellect merely because he does not get on well on the classical side, and cannot therefore be made use of with effect as the winner of a scholarship at the university—a course which some of our most noted headmasters appear too often to countenance if report belie them not. Fortunately we are not here so much the victims of educational overpressure as is Germany under the terrible influence of its military system, although there is enough to complain of, especially in the case of girls' schools, owing to the improperly large number of subjects included in the time-table; moreover, examinations, such as the London University Matriculation, are exercising a most insidious effect: and now that County Councils all over the country are granting scholarships on the results of examinations, it behoves us to be much on our guard, and to take steps to secure that all such examinations are so conducted that reasonably well-taught and reasonably intelligent scholars can be submitted to them without any interference with the normal school course. Prof. Wiedersheim, referring to the very one-sided training given in the Gymnasia to the future jurist, theologian and philologist, calls attention to the importance to such students of some knowledge of natural science in the following passage, which undoubtedly deserves our attention, as we suffer in like manner: "Kein Gebildeter vermag sich heutzutage dem Einflusse, welchen die Naturwissenschaften auf das Geistesleben aller Culturnationen gewonnen haben, mehr zu entziehen. Die ganze moderne Weltansschauung, unser Leben und Denken, die Forschung auf allen Gebieten—ich erinnere nur an das auch in der vergleichenden Linguistik zur Geltung kommende genetische und causale Element—stehen unter der Signatur der inductiven Forschung. Mit diesem Umschwung hat auch das humanistische Gymnasium zu rechnen, sollen nicht Juristen, Philologen und Theologen in ihrem ganzen Bildungsgang einen Fehler aufweisen, der oft nicht mehr gut zu machen ist." But this is nowhere properly recognised. And yet Charles Kingsley long ago dreamt of a day when every candidate for ordination should be required to have passed creditably in at least one branch of physical science—*if only to teach him the method of sound scientific thought.* Dr. Percival

[1] "Ueber die Vorbildung unserer akademischen Jugend an den humanistischen Gymnasien."—Programm wodurch zur Feier des Geburtsfestes seiner königlichen Hoheit unseres durchlauchtigsten Grossherzogs Friedrich im Namen des academischen Senats die angehörigen der Albert-Ludwigs Universität einladet der gegenwärtige Prorector Dr. Robert Wiedersheim. (Freiburg, 1894.)

"The Teacher's Manual of Lessons on Elementary Science." By H. Major, B.A., B.Sc., Inspector of Board Schools, Leicester. (Blackie and Son.)

"Practical Lessons in Physical Measurement." By Alfred Earl, M.A., Senior Science Master at Tonbridge School. (Macmillan and Co.)

"A Laboratory Manual of Physics and Applied Electricity." Arranged and edited by Edward L. Nichols, Professor of Physics in Cornell University. Vol I. Junior Course in General Physics, by Ernest Merritt and Frederick J. Rogers. Vol. II. Senior Courses and Outlines of Advanced Work, by G. I. Moler, F. Bedall, H. J. Hotckiss, C. P. Matthews, and the Editor. (New York and London: Macmillan and Co.)

has urged in Convocation at Oxford that the elements of natural science should take their place in Responsions—side by side with the elements of mathematics, and equally obligatory. The late Master of Balliol, I believe, also recognised the importance of such a change; but the reformer who will carry it into effect is not yet in evidence, and perhaps his services will not be required, as the schools must soon do that which the universities ought long ere this to have had the foresight to enforce. We are, in fact, only beginning to escape from the bonds of tradition, and it cannot be denied that our release is being gradually effected—not through any action taken by our ancient universities, but rather in spite of them—mainly through the persistent efforts of a small but untiring and resolute body of outsiders whose position has yet to be made clear to the public, most of whom undoubtedly regard the teaching of experimental science much in the way that the introduction of pianos into Board Schools was regarded a few years ago by a majority of Londoners—as something very nice for those who can afford it, but as in no sense a necessary element in the education of the masses. We, on the contrary, contend that the human mind cannot, as a rule, be completely educated by exclusive attention to literary and mathematical studies, and that lessons in experimental science must form an integral portion of the entire school course, because such lessons alone afford the means of fully developing a side of the intelligence which perhaps more than any other is of importance in life—the faculty of observing and of reasoning correctly from observation: with Kingsley, we desire that the method of sound scientific thought should be taught universally in schools, to the exclusion of dogmatism and eyelessness; and we desire to inculcate habits of self-helpfulness and handiness. The motto from Montaigne adopted by Prof. Wiedersheim fully expresses the modern view of education: "Es ist nicht ein Geist und nicht ein Körper den wir erziehen sollen, sondern ein Mensch, und wir dürfen ihn nicht theilen." Hitherto more often than not, we have cut him up into pieces, and thrown the most important aside. It is only in Germany that a public address on such a theme can be delivered in celebration of the birthday of a Royal Highness—here we must fall back on the British Association; but this body has strangely wasted the unrivalled opportunities which its organisation affords of appealing to the public on such matters.

Fortunately, help seems to be at hand from a quarter from which it was least expected, as the schoolmasters are at last awakening to the necessity of reform, and have themselves taken action which, if persevered in, must be attended with most important consequences. At the recent meeting of the "Incorporated Association of Head Masters," which now numbers nearly 300 members, on the motion of Mr. C. Stuart, of St. Dunstan's College, Catford, London, S.E., it was resolved—

(*a*) "That the Association is of opinion that Examining Bodies should encourage a more rational method of teaching science, by framing the syllabuses in such a manner that the practical work required may be strictly illustrative of the theoretical instruction given."

(*b*) "That it be referred to the General Committee to appoint a small Sub-Committee, so that a report may be presented to the next summer general meeting containing detailed suggestions, which it is proposed to make to Examining Bodies concerning examinations in science."

As I ventured to point out, when speaking on these resolutions, the consequences of their adoption will probably be far greater than those who have accepted them are likely to have contemplated. Thoughtful examiners have long been waiting for a sign: no one has been more dissatisfied with their examinations than they themselves have been, but it has been impossible for them to examine much in advance of the teaching, as it would obviously have been most unfair to candidates to make them victims of a system for which they are in no way answerable; and at most it has been possible to gradually give a practical turn to questions in subjects which can only be taught properly by means of practical lessons. The gage thrown down by the headmasters will, therefore, certainly be uplifted forthwith by all examining bodies whose work is not done in a purely perfunctory spirit, and the schools will find that of their own accord and most properly they have brought about a complete revolution in their own methods of teaching; for if they ask for proper examinations, they necessarily must be anxious to see corresponding proper methods of teaching adopted in their schools, and will provide for their introduction. Nothing could be more gratifying to those among us, who, during years past, have been pointing out the necessity of introducing radical changes and improvements.

A serious responsibility will now be cast on teachers, and it is clear that if workers are forthcoming with minds imbued with the proper spirit of inquiry, there will be ample opportunity of gaining distinction in the field of educational research. There is clearly much leeway to be made up, for although we are all agreed that every branch of natural science must be taught practically, we are, unfortunately, far from always practising what we preach—the system of mere lesson learning not only prevails far too widely and extensively, but is far too frequently regarded with approval as all-sufficient. The practical and theoretical work are seldom, if ever, made sufficiently interdependent—in fact, this is the blot on which the headmasters have laid their finger. Even in my own subject, chemistry, which is generally supposed to be in a stronger position than most others, as it has been taught practically from a considerably earlier date, much misconception prevails, and we are credited with having advanced far more than we deserve. Liebig, the founder of the laboratory method of teaching, appears to have taught *chemistry*—we are told that when Hofmann became a student under him, although a beginner, he was set to work at research; and we know that Liebig gave all his students problems to solve. But the march of progress and, especially, the press of numbers have long since led to the introduction of the anti-research method which some of us irreverently term test-tubing. Such and such *is* the case—do so and so, and *if* you do so and so, this and that *will* happen, are instructions so often dinned into beginners' ears, that they become part of their very being, and they in consequence are ever afterwards unable to give a straightforward account of what *they* have done, and insist, instead, on telling you, the teacher, what to do and what will, not what does, happen. Nothing is more rare in a chemistry examination paper than a straightforward answer, not in terms of if and will, showing that the writer is describing from personal knowledge something that *has been* done. The true object of experimenting is thus lost sight of as the habit grows of requiring to know in advance what will be the outcome of any particular experiment: the spirit of curiosity is rarely awakened. At the same time the worst possible literary style is encouraged, a real opportunity of developing a good one being most perversely sacrificed. Analytical tables were originally introduced with the laudable object of presenting knowledge to the student in a systematic form, but gradually we have allowed them to pass from the position of good servants to that of bad master—mainly because they offer so very convenient a method of keeping students occupied with a minimum of attention and labour on the part of the teacher and at a minimum cost, as a few test-tubes and weak solutions form almost the only stock in trade. Quantitative work is also done in an almost equally per-

functory way, as a rule, with the object of qualifying the student to do technical analyses, and the importance of measurements in establishing first principles is never taught practically, but is merely talked about: the result is that only the few who study the subject professionally, really grasp the meaning of the fundamental quantitative conceptions of our science. It is not surprising that under such a system, being helped over every stile, and having no training in research methods, our students are so very rarely properly educated. And altogether false ideas have arisen also as regards the value of true research work, this having been allowed in far too many cases to degenerate into mere preparation-making, or mere measurement work. There are many among us who have recognised these shortcomings in our method, but tradition exerts its all-powerful influence, and little short of a revolution is required to reintroduce a truly scientific procedure into our schools and examinations—to lead teachers to recognise that the only proper method is to make students researchers from the very outset. These remarks are but prompted by the desire to acknowledge that if I throw stones, I am fully aware that I dwell in a glass house; and especially to make it clear what is the point of view from which I regard the problem before the teacher of any branch of natural science.

The teaching of physics practically is of quite recent introduction. When I was a student at the Royal School of Mines, there were only lectures on physics; and when, about fourteen years ago, my colleague Prof. Ayrton and I visited all the chief continental schools, we found the practical courses in a very embryonic state. Now, although we have nowhere a laboratory which will compare in size or completeness of fittings with the palace erected at Zürich, practical physics is taught in all science schools and colleges, and the London University requires even candidates at the Intermediate Science and Preliminary Scientific (M.B.) examinations to pass a practical test. Many of the courses are very complete. I shall not easily forget the pleasure which I experienced on the occasion of a recent visit to Prof. Quincke's laboratory at Heidelberg from seeing there the marvellously simple, but yet exact, apparatus used in the practical course; the insight into the inner meanings of things afforded by his arrangements struck me as most perfect, and led me to wish that it were possible to teach my own subject so as to give the course an equally high educational value. There are also nowadays many admirable books from which the student may gather instructions how to make experiments in the various branches of physics—there can be no doubt, in fact, that the practical text-books in this subject are generally of a very high quality. But I venture to think that they need modification in some not unimportant particulars. Whilst in advance of the practical books at the disposal of the chemist, inasmuch as they stand in direct relation to the instruction in theory, being intended, as a rule, as Prof. Nichols happily states in the introduction to his first volume—to illustrate and therefore impress more thoroughly on the mind the principles and laws which have previously been taught by text-books or lectures, yet for this very reason the attitude in which they place the student is a wrong one. A "law" is dogmatically stated, and the student is told how to verify it experimentally, so that the young worker, instead of being led to *inquire*, is perpetually told in advance what are the facts, and is instructed to repeat the experiments merely in order to *prove* certain things. Consequently, in physics as in chemistry, students are far too little encouraged to find out things and to help themselves—instead of becoming imbued from the outset with the spirit of inquiry, they are led to expect and require assistance at every step. While galvanometer needles wag, they calmly put their hands into their pockets, and it becomes very difficult to induce them ever to adopt any other mental attitude. Whilst therefore our teaching is enormously improved by bringing students into personal contact with the facts to an extent altogether undreamt of even in my student days, by the very wealth of appliances now at their disposal, they are fast becoming spoilt—perfect sybarites, and the self-helpfulness engendered by the rude and scanty apparatus of days gone by is strangely infrequent. The system apparently fails to engender or develop originality and powers of observation, however much it may tend to train even well-informed and exact workers; but I imagine that little more than a change in the form of the instruction is required, in order to make it impossible to say of any one, "that he knows *about* all sorts of things, but he can't *do* them," the only text, it seems to me, worth preaching from at present. The principle it embodies is the one of all others upon which the whole practice of education must be built up, whatever the subjects taught; but it is undoubtedly one which has not been kept persistently under notice, as it should have been by teachers generally.

The habit of mind complained of, so characteristic of all but a few gifted individuals among our students, is probably largely engendered by the training they have received during the early years at school; and it is incumbent on all teachers working in the field of educational research to do their utmost to develop methods which will counteract the evil effects of mere lesson learning and desk work, as these cannot be altogether dispensed with.

As illustrating—how not to teach elementary science, the "Manual" first on my list must be assigned to a high place; no doubt the intention is good, the information may be interesting and useful in its way, although often very bald if not inaccurate, but such a book has no right to figure as a "Manual of elementary science." It comprises instructions for object lessons, on every possible topic, to children in the six standards of elementary schools, in the form of very short chapters either of "special information for the teacher" or "introductory specimen lessons," and of "notes of lessons" in which, in parallel columns, the kind of question to be asked is set out under "method," and the information to be imparted under "matter." As in all books of this class, far too much is attempted. In the hands of a really capable teacher—who would not need such a book, however,—object lessons may be made of the highest value, but even in such hands the tendency always is mainly to impart information; the kind of lessons that would be given by the uninstructed teacher gaining inspiration from Mr. Major's manual, is easier imagined than described, and their educational effect could not fail to be harmful. We are better without such "science," and had better stick to the plain bread and butter fare of "prescientific" days, if we cannot teach in such a way as to inculcate the practice of scientific method; in fact, we do not want "science" teaching of any kind introduced into elementary or, indeed, any schools unless it take the form of work done by the children themselves: in no other way can the end we have in view—that of training them to do, not merely to know—be achieved, and any other kind of teaching must be a pretence and but an encouragement of priggishness.

As regards the course of the future, there is no doubt that much may be done in very early days to lead children to take note of everything about them, to describe what they see, and to collect and describe common materials of every kind. Some slight preparation for the study of botany may also be laid at this stage. But the science lessons proper begin when the children know enough arithmetic to measure and weigh. All who have studied the problem practically are probably agreed that simple measurement lessons must form the foundation, and there cannot be a doubt that it is both

desirable and possible to largely incorporate these with the arithmetic lessons—to teach parts of arithmetic and some geometry practically, in fact. Gordon's "Elementary Course of Physical Science" is probably the type of book which will be used with advantage at this stage, judging from the success the course has met with in Board Schools commencing with the fourth standard.

But the pioneer worker in this field is Prof. Worthington. His admirable little book, "An Elementary Course of Practical Physics," published in 1881, well known to all teachers of physics in schools, was expanded in 1886 into a larger work, "A First Course of Physical Laboratory Practice." These books have been of the very greatest service, and have probably rescued physics from being made a cram subject in schools; but they are scarcely simple or comprehensive enough for such young workers as I am contemplating. As they are intended to be used in connection with lectures, the motive for each experiment is rarely explained at sufficient length, and unfortunately they have the fault common to all such books, to which I have referred above, that the student rarely approaches the experiment in the attitude of the would-be discoverer.

Mr. Earl's book, although it deals only with measurements of length, mass, and time, is of greater length than Prof. Worthington's, but it does not need lectures to supplement it. It has a short but admirable preface, showing that the author has grasped the nature of the problem to be solved, but it is one thing to do this and another to succeed in solving it; and here I think he has been less successful. The style is far too didactic—the descriptions are far too elaborate—the aim is too high, and far too much is told, not enough left to be found out: to use a homely phrase, which I trust will not be misunderstood, far too much fuss is made about the work. Such work should be done by young children; it must not be postponed until the evil effects of desk work have warped the scholar's mind, and natural curiosity begins to die out. But the book is far above such students. What, for instance, is the value of an introduction telling us what science is, and why we learn it? If taught to work, children will very soon insensibly learn to appreciate what they are doing, and nothing is gained by pointing out to them, for example, that "each individual stands a centre for himself of all things, knowing that around him are centres of endless variety, &c." Platitudes such as these are out of place in an elementary text-book, and they are beyond children of nine to twelve years of age, by whom simple measurement work will ere long be done in schools generally. In the next chapter, in like manner, we find at the outset a somewhat elaborate disquisition on the meaning of the words standard and quantity, and a description of how measurements of length are made, and then follow directions for a series of exercises: children cannot grasp such disquisitions, and are wearied by them, but most of them will use a foot or centimetre rule with great pleasure. This criticism may be applied to the book generally—the introductory instructions are too frequently high-flown, and too many refinements are introduced into them and into the exercises. The book, in fact, contains material enough both for a junior and a senior course, and would be more generally available if the matter were thus rearranged. But whilst objecting to it on the score of over-elaboration and absence of that simplicity of statement which is indispensable in a book to be used by young beginners, taken as a whole, and regarded from what I conceive to be the author's point of view, it is a contribution to educational literature of great value. The short preface alone is a manifesto of singular importance, coming as it does from a teacher in a public school of high standing. We find in it the remarkable and, I believe, novel expression—the "scientific sides" of Public Schools, one, it may be hoped, that will soon take the place of "modern side," to which so great a stigma is attached through the ill-advised action of masters having sympathy only with classical training—action which has led to the modern side being often characterised as the refuge for the intellectually destitute; if the modern side once become the seat of training in scientific method, a fair share of the intellect of the school being allowed to it, there cannot be a doubt that it will soon reap payment "by results" sufficient to secure to it an honourable revenge. To return to the preface, we read that care has been taken to make the course logically progressive, the end in view being to train boys to observe accurately, to reason rightly, and to front nature with an open and inquiring mind. That it must be admitted more generally than is customary that the retention of facts should be subordinate in scientific education to a sound comprehension of them, a mind which has been trained to observe and compare accurately being more likely to acquit itself well in the world. That the right way of learning is chiefly to be cultivated, the matter being obviously less important than the method in learning science, and a logical and inquiring habit of mind being more valuable than the memory of facts and laws, as it is better equipment for future research and knowledge. It is urged that the course should afford some training in correctness of expression and accuracy of language, no insignificant part of scientific education, although this is seldom recognised. Lastly reference is made to the lack of continuity in work often prominent as a defect, especially in the modern sides of schools, and to the fact that much of the average "capacity of learning" is never utilised, owing to frequent change both of subject and matter. The book may serve in some degree, the author suggests, to bridge over with safety the distance between the laboratory and other classrooms, by acting as a Practical Arithmetic and, to some extent, as a Practical Grammar. Trite as some of these remarks may appear to be, those who are aware of the state of affairs in schools will know that, tested by the standards which generally prevail, they are the rankest heresy, but none the less heresy that must ere long revolutionise existing beliefs and practices. In fine, I am inclined to regard Mr. Earl's book as the account of an important research some results of which he has described in a previous publication, this being his second appearance as the author of an educational work. There is the clearest evidence that he has set himself to work out a problem of which he was able to realise, perhaps, only the general nature at the outset; and that, as so often happens in experimental inquiries as experience is gained, the nature of the underlying problem has been more clearly grasped as the work progressed, and important modifications of procedure have consequently been introduced. I look forward with expectancy to his next memoir, in which the results of his further studies will be presented to us, I trust, in far less elaborate guise, so as to render them available to the simplest understandings.

"The Laboratory Manual of Physics and Applied Electricity," arranged and edited by Prof. Nichols, of Cornell University, U.S.A., is intended as a college course, and is stated to be the outgrowth of a system of junior instruction which has been gradually developed during a quarter of a century, and I shall therefore venture to criticise it broadly from an outsider's point of view, as though it were a report on the results of an inquiry, with reference to its general character and the arrangement of the subject-matter. The first volume by Messrs. Merritt and Rogers is intended for beginners, and, we are told, affords explicit directions together with demonstrations

and occasional elementary statements of principles. It is assumed that the student possesses some knowledge of analytical geometry and of the calculus; also that he has *completed* a text-book and lecture course in the principles of physics. Here we may at once join issue with the authors. If, as is so frequently and indeed almost always the case at present, the student come to college knowing a good deal of mathematics and no physics, the first assumption is a legitimate one, but it is time that we began to insist that such educational abortions should not be reared, and that girls and boys be no longer allowed to grow up so ignorant that, although they have learnt a good deal of mathematics, they require to be told, when they come to college, how to use squared paper, how to make simple experiments illustrating the principle of the parallelogram of forces, how to roughly determine density by weighing in water and by means of a bottle—all of which, as well as a number of other practical arithmetic exercises for children, are included by Prof. Nichols in his first volume. The incongruity is made still greater by the appearance in the introduction, ten pages after the description of the graphical method of representing results, of a somewhat elaborate discussion of "probable error" and the application of the method of least squares. Surely, also, no student nowadays should be allowed to complete a text-book and lecture course before entering on practical work—are we not seeking rather to invert this order? Were it not that the editor tells us that it is not expected that the experiments will be taken consecutively, the book might be regarded as helping teachers in the choice of a properly graduated course of experiments; and with this confession before one, it is impossible also to take exception to the somewhat extraordinary classification of the experiments—an instance of which is afforded by placing an exercise on the measurement of the curvature of a lens by means of the spherometer first in the course, at p. 26, whilst the determination of relative density "roughly" by weighing in water is relegated to chapter ii. p. 80. Some indication of the editor's experience as to the order in which the experiments are advantageously arranged in a junior course would have been valuable, as even to meet the exigencies created by the simultaneous presence of a large body of students, it is scarcely desirable to take physical exercises in any order. Again, we are told that no attempt has been made to provide a complete and sufficient source of information for laboratory students—on the contrary, it has been thought wise to encourage continual reference to other works and to original sources, so that it is impossible to complain of the book on the score of the incompleteness of many of its descriptions.

The second volume is more interesting, logical and original, and less fragmentary. It is intended for students who have completed the "course" in vol. i. and who are prepared to take up special work; in it the needs of those who are in training to become electrical engineers have been specially considered—a sign of the times—and it is for them especially that the parts treating of applied electricity, heat, and photometry have been written. Parts i. and ii. occupy about half the book, and comprise 53 direct and 64 alternate current experiments, the intention clearly being to frame a systematic course suited to those who have gone through a simple qualifying course in general physics. The instructions are very tersely worded, and but serve to give general guidance—which is very desirable in the case of students in whom it is desired to develop habits of independence. Immediately after the electrical engineering course, there is a very short section on electric-light photometry. Following this comes the least satisfactory section in the book, that on heat—dealing with the experimental determination of specific heats, heat of combustion, vapour pressure, vapour density, heat of vapourisation, mechanical equivalent of heat, cubic expansion, and measurement of temperature by means of thermoelectric couples. Much of this might with advantage be placed in the qualifying introductory course, and the engineering student would gain little from the exercises which do not properly find a place there. Moreover, this section is grievously behind the times in many respects—under vapour density, not only is the sole method described "the classic one used by Dumas," but the picture is that which has figured in every text-book since Dumas' description was published in 1826, showing a stove which it would be impossible to find. Experiment 9 is headed, "Use of the Favre and Silbermann water calorimeter," an instrument which it is difficult to imagine *in use* by students. But Mr. Matthews, who is responsible for this section, is clearly not a Rip van Winkle, as he also describes the Bunsen ice calorimeter and Berthelot's heat of vapourisation apparatus.

Part. iv. of the second volume comprises outlines of advanced work in general physics, based chiefly on researches done in the physical laboratories of Cornell University, written for the use of students who have completed the routine work, and desire to enter upon original investigation. This section will scarcely be of value to outsiders, as every competent head of a laboratory has problems enough awaiting solution, and a text-book of research is scarcely a much-felt want. Excellent advice is given at the outset of this section, and yet it is, in part, of such a kind as to bring out only too clearly how much we are behind in our conception of what is right and proper in education. At this stage—the commencement of original investigation—says Prof. Nichols, there is a critical point. Further success depends upon several matters which have been necessarily somewhat neglected during the earlier periods. In the first place, the student must acquire independence and self-reliance; he himself must face the experimental difficulties of the problem upon which he may be engaged, and must overcome them by devices of his own. Surely, we ought not to wait so long—the first essential in teaching scientific method should be to develop habits of independence; and if from the outset we lead students, as far as possible, always to take up the attitude of the discoverer, there should be no critical point in their career, but they should insensibly escape from leading-strings and walk alone.

Being prepared specially for students at Cornell University, the book will scarcely be used very widely, and, indeed, is not suited for general use, I imagine; yet it appears to me to be of considerable interest to teachers, as marking a distinct step forward in the evolution of the text-book of the future. There can be little doubt that those students, at all events, who have a technical career in prospect, will be taught on lines such as are indicated by the arrangement of the subject-matter adopted by Prof. Nichols—that after going through a qualifying introductory course, in which they gain a general understanding of the main principles and of method, they will devote their attention to the technical study of whatever branch they may select. But it is to be hoped that they will be so taught at school that the introductory college course will not only be much shorter than is at present necessary, but of a much higher character; and that, as I have said above, they will be inquirers from the outset.

To carry such a scheme as I contemplate in this article into execution, however, teachers of all subjects must be trained in the school of research: surely this is the keynote of future progress in education, indeed of our national progress.

HENRY E. ARMSTRONG.

NOTES.

THE Council of the Royal Society have fixed Wednesday, May 1, for the first Soirée of the Session. The Ladies' Conversazione will be held on some convenient day in June, probably the 12th.

WE understand that Sir Joseph Lister, Bart., the Foreign Secretary of the Royal Society, will be nominated for the presidency of the Liverpool meeting of the British Association, to be held in 1896.

PROF. W. C. UNWIN, F.R.S., will deliver the "John Forrest" lecture this year, at the Institution of Civil Engineers, on "The Development of the Experimental Study of Heat Motors."

A SERIES of Cantor Lectures, on Commercial Fibres, will be commenced at the Society of Arts, John Street, Adelphi, on Monday next, by Dr. D. Morris, C.M.G. The subsequent lectures will be on March 25 and April 1.

THE 1895 conference of the Camera Club will be held on April 2 and 3, under the presidency of Captain W. de W. Abney.

THE death is announced of Sir Edward Bunbury, the author of "History of Ancient Geography." Mr. Alfred Giles, whose name is well known among engineers, has also recently died.

AT the meeting of the Royal Meteorological Society, to be held on Wednesday, the 20th instant, a lecture will be given by Mr. W. N. Shaw, F.R.S., on "The Motion of Clouds considered with reference to their Mode of Formation."

THE Council of the Society of Arts invite members to forward to the Secretary, on or before April 13, the names of such persons as they may think worthy of the Albert Medal for 1895. The medal, which is given annually in recognition of "distinguished merit for promoting Arts, Manufactures, or Commerce," will be awarded in May.

IT is interesting to note, in connection with the subject of Mr. Stromeyer's letter published last week, that Reuter's correspondent at San Francisco reports that vessels arriving there announce the occurrence, on the 2nd inst. of an earthquake in the bed of the Pacific Ocean. The disturbance was accompanied by a loud roar, coming, apparently, from the sea, which became covered with a mass of white foam, and subsequently rose in numerous geyser-like columns.

MR. HYDE CLARKE, the author of numerous works and memoirs on philology and mythology, and a member of the Council of the Anthropological Institute, died at the beginning of this month, in his eightieth year. So long ago as 1836 he planned, as engineer, and also surveyed, the Glasgow and South-Western Railway, with the Morecambe Bay Embankment. He was engaged a few years later in acoustic telegraphy, and was employed to report on the telegraph system for India. His writings cover a wide range of subjects, but he was best known for his contributions to philology.

OUR attention has been drawn to a scheme, which owes its initiation to Canon Tristram, and for which further sympathy and support is needed. With the object of creating a more general interest in the study of Natural History, and to stimulate observation and research, it has been proposed to establish a fund for a medal or other prize to be given annually by the Natural History Society of Northumberland, Durham, and Newcastle-upon-Tyne, for the encouragement of field work and original observations in any branch of the subject, whether Botany, Geology, or Zoology. The prize will be offered for the best essay evincing intelligent study of any of the common objects noticed in the fields or woods, on the moors, or by the sea-shore, &c.; competitors to be residents in the counties of Northumberland or Durham, or in Newcastle-upon-Tyne. It has been determined to associate the medal with the name of the late Mr. John Hancock. The capital fund required to provide the medal will not be less than £200, towards which amount over £100 has already been given and promised. Subscriptions towards the fund may be sent to Mr. A. H. Dickinson or Mr. M. C. Potter, Hon. Secretaries of the Natural History Society, Newcastle-upon-Tyne.

THE intention of the Ottoman Government to establish a geodynamic observatory at Constantinople has already been noticed in these columns (p. 180). The new institution forms a section of the Imperial Observatory, and is under the direction of Dr. G. Agamennone, who has just issued a valuable list of earthquakes felt throughout the empire in 1894. The chief interest of this catalogue centres in the accounts of the great earthquake of July 10, and the detailed record of its aftershocks. In the Observatory registers there are also preserved many seismic notices relating to previous years, which are to be edited and published at the earliest opportunity.

THE seismic history of several districts in Italy has been worked out with great care and thoroughness by Dr. Mario Baratta. In his last paper (*Rivista Geogr. Ital.*, Gennaio, 1895), he treats of the earthquakes of Calabria Ultra. In this province there are five well-marked seismic centres, or radiants. From their position, and from the direction in which the epicentre is displaced, it is clear that they are closely connected with the prominent faults which have helped to shape the present surface features of the district. Dr. Baratta concludes that the earthquakes of 1783 were produced by vibrations of these faults, and that the shock of last November 16 was a repetition of the terrible disturbance of February 5, 1783, the areas most strongly affected by them both being almost or quite identical.

THE Council of the Marine Biological Association has authorised the expenditure during the present year of a considerably larger sum for boat hire than has been spent in previous years. Naturalists who visit the laboratory during the coming season may thus expect increased facilities in their work, and ample supply of material for their researches. The Council, moreover, has authorised the director to offer free tables in the Plymouth Laboratory to naturalists who will be willing to render assistance in the arrangement of the collections in the museum, and in the collecting operations at sea. It is to be hoped that many will take advantage of the efforts which are being made to extend the usefulness of the magnificent laboratory at Plymouth.

THE Prince of Monaco has recently reported upon the first scientific cruises of his yacht the *Princesse Alice*. The area investigated included the western basin of the Mediterranean, the Straits of Gibraltar, and the Gulf of Gascony, along the western coasts of Morocco, Portugal, and Spain, down to a depth of 4898 metres. Fifty-eight soundings and forty-six temperatures and samples of sea-water were taken in these localities. The trawlings made by the *Princesse Alice* in the western basin of the Mediterranean merely confirm the known poverty of the fauna of the great depths of this part of the Mediterranean; the Atlantic operations of the ship in 1894 were unfortunately impeded by prolonged and violent northerly winds.

MR. J. Y. BUCHANAN, who has investigated the samples of water taken by the *Princesse Alice*, finds that the density of the Atlantic water is the same along the whole south coast of Spain as far as Cape Gata, owing to the existence here of a strong surface current setting eastwards. From Cape Gata eastwards

only the denser water of the Mediterranean proper is met with. The difference between the ratios of salinity to alkalinity in the case of the Atlantic and Mediterranean waters is not very great, but is clearly marked. The mean coefficient of the Atlantic samples examined by Mr. Buchanan was 0·5000, and of the Mediterranean 0·4875; while the minimum coefficient of the Atlantic samples was greater than the maximum coefficient of the Mediterranean. A possible cause of the difference between the Mediterranean and Atlantic waters is sought in the abundance of calcareous rocks on the coasts of the former sea.

THE first number of the new Chilian *Journal of Hygiene* has recently been issued. It is published under the direction of the Institute of Hygiene recently established in Santiago, and is printed in Spanish. The present volume confines itself to the history and development of the organisation of public hygiene in general throughout Chili, and an account is also given of the provision for official hygienic administration in Germany, France, England, and Belgium. One of the functions exercised by the Santiago Hygienic Institute is the analysis of substances for purposes of trade and commerce, and the granting of official certificates as to their quality. Considering the enormously high death-rate of Santiago, which is stated to reach 57 per 1000, some reform in hygienic matters is urgently needed, and it is to be hoped that the establishment of this new Bureau of Public Health may beneficially stimulate public and private enterprise in this direction.

WE could not have a better example of the insufficient morphological descriptions which have become attached to some micro-organisms, than that afforded by Mr. A. Coppen Jones in his memoir on the tubercle bacillus, lately published. This investigator has studied this micro-organism for several years past at Davos, where he has had ample opportunity for the collection of tuberculous material, and, after most elaborate and painstaking researches, he considers that we are not yet in a position to assign its correct morphological place to the organism associated with consumption. Mr. Jones is of opinion that it probably stands in close relation to the moulds, and that it has far more right to be regarded as belonging to this category than to the schizomycetes in the strictest sense of the word. The conclusions are based upon very careful observations, and the memoir is abundantly furnished with beautiful illustrations. The tetanus bacillus has also been seen associated with mycelium threads, and Almquist is stated to have isolated two bacterial forms which produced a branched mycelium. The subject is a most interesting one, and well worthy the attention of morphologists.

AT the meeting of the French Meteorological Society on February 5, an important discussion took place on the subject of anemometry and the vertical currents of the atmosphere, which have sometimes been recorded by the so-called clino-anemometer. M. Ritter thought that the ascending currents registered by those instruments were simply due to deviations of the atmospheric currents by obstacles, such as the tower on which the recording instrument was placed, which divides the current laterally, and also inflects it in an upward direction. M. Angot stated that such vertical currents do not rise to any great height, in proof of which he said that on making the ascent of the Pic du Midi, with an ascending wind caused by striking the incline of the mountain, a number of small papers thrown into the current only rose a few metres, and then fell down behind the observer. These observations tend to modify to a great extent the results of experiments on vertical atmospheric currents, which have from time to time been published, and make it advisable for further experiments to be carried out.

THE Report of the Meteorological Council for the year ended March 31, 1894, has been recently presented to Parliament. In the department of ocean meteorology, the preparation of current charts for all oceans have been regularly proceeded with; the Council have procured from foreign meteorological offices nearly all the observations for the Pacific Ocean, with the view of rendering the generalised results as trustworthy as possible. The preparation and extraction of data for the south Atlantic and west coast of South America, have also been actively prosecuted. In the branch of weather telegraphy and forecasts, an important addition has been made by the receipt of daily reports from two stations in the Azores. Forecasts are prepared three times a day, either for publication in newspapers, or for the preparation of storm warnings, while during the hay-harvest season, forecasts were supplied to farmers and others, and were also widely distributed by the Board of Agriculture. The total percentage of success of these agricultural forecasts reached 91, being the highest yet recorded. Among the various publications the *Weekly Weather Report* supplies a very complete view of the chief meteorological changes over the greater part of Europe; considerable progress has also been made with the rainfall tables of the British Isles, the total number of stations for which the values for 1881–90 will be furnished is approximately 430, so that the general distribution of rainfall over our islands will be well represented. Among the miscellaneous researches carried on by the Council, we may mention the comparison of Dines's pressure tube anemometer, which shows the wind pressure at each instant; the vane is erected 93 feet above the ground, and the recording portion is in a room in the Meteorological Office. Upon the whole, the comparison speaks strongly in favour both of the reliability of this instrument, and of the mean record yielded by the Robinson cup anemometer, which has hitherto been generally in use, both in this country and abroad.

IT has often been observed that a bright scarlet uniform will in a good photographic dark-room with ruby-glass windows, appear perfectly white. On this subject Herr H. W. Vogel made some interesting communications to the Physical Society of Berlin at a recent meeting. Experimenting with oil lamps provided with pure red, green, and blue colour screens, he found that when white light was rigidly excluded, all sense of colour disappeared to the observers, and nothing but shades of black and white could be distinguished on objects in the room. He further found that a scale of colours illuminated by red light showed the red pigments as white or grey, which abruptly turned into yellow, and not red, on adding blue light. Hence a colour was perceived which was not contained in either of the sources. Red and yellow patches appeared of the same colour, so that they could hardly be distinguished. But the difference was at once brought out by adding green instead of blue light. How very much the kind of sensation experienced depends upon the intensity of illumination is easily seen in the case of the region of the spectrum near the G line of Fraunhofer. This region appears violet when its luminosity is feeble, blue when it is stronger, and may even appear bluish-white with strong sunlight, so that the assertion often made that with normal eyes a definite colour-sensation corresponds to a definite wave-length, cannot be upheld. Herr Vogel comes to the conclusion that our opinion as to the colour of a pigment is guided by our perception of the absence of certain constituents. Thus a red substance is only recognised as such when light of other colours is admitted, and we perceive its inability to reflect these.

AT the meeting of the Institution of Civil Engineers, on Tuesday, March 5, two papers, dealing with the transmission of power by electricity, were read. The first, on "Electrical Haulage at Earnock Colliery," by Mr. Robert Robertson, contained an account of the general features of the colliery and of

those of its seams into which electrical hauling-engines had been introduced to drive endless ropes, and to replace horse-traction between the main haulage-roads and the working-faces. The electrical generating plant comprised a dynamo for supplying power to the electrical hauling-engines, capable, at a speed of 620 revolutions per minute, of an output of 100 amperes at 490 volts. Two electrical hauling-engines, of which the main features were similar, were used. The two cables of stranded steel wire $\frac{3}{4}$ inch in diameter, operated circuits of 2160 yards and 1020 yards in length respectively. The cables were driven continuously at a uniform speed, and the coal-hutches were attached to or detached from them by a Smallman clip. The efficiency of the plant as compared with the horse-traction, which it had replaced, was also considered. The electrical haulage-system in the Ell coal seam was capable of a daily output of 400 tons. The daily output by horse-traction had been 180 tons, and to increase this to 400 tons, thirty or forty horses would have been required, a number which could not have been employed in the available space without confusion. The daily output of the electrical haulage-system in the main coal-seam, which was not yet working at its full capacity, was between 150 and 200 tons. The yearly working expenses of the two systems were compared, upon the results of one and a half years' working, and were found to be £4130 and £1990 by horse-traction and by electrical haulage respectively, showing that an annual saving of £2140 had been effected by the latter. The total cost of the electrical installation had been £3500.

IN the second Paper—"Water-Power applied by Electricity to Gold-dredging," by Mr. Robert Hay—an account was given of plant which had been erected in New Zealand to utilise water-power for generating electricity to be transmitted to motors operating a dredge in different portions of a distant river. The plant described had been constructed for gold-dredging in the River Shotover, the course of which was for the most part in rocky gorges through rugged country, accessible only by tracks cut down the leading spurs and gullies. The water was obtained at a creek $1\frac{1}{2}$ miles distant from the dredging ground, and was brought by a race cut in the side of the hill, or, in places where the ground was not suitable, in a timber-flume, to a pressure tank at a level of 524 feet above the pipes at the generator-house. From this tank to the wheel employed the water was carried in rolled-steel pipes. The prime-mover of the generating plant was a Pelton reaction water-wheel, upon the buckets of which the water, from a nozzle $1\frac{1}{2}$ inches in diameter, impinged at a pressure of 228 lbs. per square inch. This wheel drove two series-wound dynamos working at a normal speed of 700 revolutions per minute, each developing a current of 40 amperes at an electromotive force of 650 volts, or together nearly 70 h.p. The conductors, of a length of two miles, were of No. 4 S.W.G. bare copper wire, and were supported upon insulators carried by cross-arms upon old 40-lb. rails. The current was conducted to two motors in the dredge, one for driving a centrifugal pump, and the other for operating the buckets, winches, and revolving cylinder. Two 10-ampere arc-lamps lighted the dredge at night, and were joined in multiple-series with the motors, with suitable arrangements for their control. The cost of the installation and the weekly working expenses were £7000 and £35 respectively.

AT a recent meeting of the Academy of Science of Amsterdam, M. Kamerlingh Onnes presented a paper by M. L. H. Siertsema, on magnetic rotary dispersion in oxygen. Since making a preliminary communication on this subject, the author has improved his apparatus in several details, and is now able to examine the gas under a pressure of a hundred atmospheres. The oxygen employed was prepared by electrolysis. The measurements of rotation are made by the well-known method in which white light, having passed through the polariser, observing tube, and analyser, is then examined by means of a spectroscope. A dark band is thus obtained traversing the spectrum, and the analyser is turned till this dark band coincides with the part of the spectrum corresponding to light of any desired wave-length. With currents between 35 and 65 amperes, the author is able to get rotations of from 3° to 4°. In the case of oxygen the results are very well expressed by the formula $\gamma = \frac{868 \cdot 028}{\lambda}\left(1 + \frac{0 \cdot 07202}{\lambda_2}\right)$ where λ is the wave-length in thousandths of a millimetre; the mean error in γ being $\pm 17 \cdot 5$. This formula is deduced from the formula $\gamma = \frac{c}{\lambda}\left(n - \rho\lambda\frac{\delta n}{\delta\lambda}\right)$ where n is the refractive index, given by Mascart. A series of measurements were also made on atmospheric air, and the values for nitrogen deduced by comparison of these numbers with those obtained for oxygen. The expression thus obtained for nitrogen is as follows :—

$$\gamma = \frac{560 \cdot 41}{\lambda}\left(1 + \frac{0 \cdot 32424}{\lambda^2}\right).$$

AN interesting series of experiments have been carried on by Mr. T. Andrews, on the influence of stress on the corrosion of metals, which may have considerable practical applications. Bars of iron or steel were subjected to different stresses, and were then placed in some electrolyte—generally a solution of common salt—and the difference of potential between them measured. It was found that tensile stress caused the production of an average E.M.F. of 0·016 volt between the strained and unstrained bars, the latter being positive. The effect of torsional stress was to produce an average E.M.F. of 0·012 volt in the same direction. Similar results were obtained with flexional strains. From the data obtained, the author deduces a number of interesting conclusions which bear on the corrosion of iron structures, for which we must refer to the original paper in the *Proceedings* of the Institution of Civil Engineers, cxviii. 4.

THE ever-increasing precision in the study of the geological distribution of fossils renders it necessary, from time to time, to recognise that a well-marked lithological division, though of the greatest value in geological mapping, does not represent a definite time-level, but may gradually rise or fall in time as it is traced along its outcrop. This fact is well illustrated in England by the Cretaceous beds underlying the true Chalk, and some of the questions involved have been discussed in two recent papers in the *Geological Magazine*. The first of these, by Messrs. Jukes-Browne and Meyer (November, 1894) deals with the Upper Greensand and Chloritic Marl. They point out that the latter term has been applied—and may justly be in its lithological sense—to various horizons up to the zone of *Belemnitella plena* (as at Beer Head), and they propose that if the name be retained it should be strictly limited to the zone of *Stauronema Carteri*, which may be regarded as the bottom bed of the true Chalk. They further point out that the well-known Warminster fossils are a mixture of two faunas—the majority coming from the summit of the true Upper Greensand, just below the *Stauronema* zone, though some are from that zone itself, the state of preservation being different in the two cases. Thus the confusion between Chloritic Marl and Warminster Greensand is due to a mixture of fossils from different levels—an accident that has often caused confusion in other formations—and a complete revision of the lists of fossils from these horizons must precede any further correlation. The second paper, by Dr. J. W. Gregory (March, 1895), deals with the Gault and Lower Greensand. After considering evidence of various kinds, he comes to the conclusion that the conformable passage from sandy beds into clay rises in time as it passes westward from Kent to Surrey, much as the upper limit of the

clay sinks further west towards Wiltshire. The lowest zone of the Gault in Surrey that has the typical clayey facies is equivalent to the fourth or higher zone of the Folkestone Gault, the lower zones being represented in the so-called "Folkestone Sands." Dr. Gregory indicates the levels at which, in Kent and Surrey respectively, the limits of the Albian, Upper and Lower Aptian, and Rhodanian formations of the Continent can be drawn; but insists on the necessity of continuing to recognise the old lithological divisions side by side with the new palæontological ones. He also gives a critically revised and extended list of fossils from the phosphate beds of Great Chart, near Ashford, Kent.

THE 1894 "Report of Observations of Injurious Insects and common Farm Pests," by Eleanor A. Ormerod, will not yield to any of its predecessors in interest, value, or variety. The general character of these Reports is so well known that it is unnecessary to dwell upon it; but the present part gives prominence to several considerations which have not hitherto received very much attention. The first of these is the undoubted fact that almost any of the 12,000 species of insects inhabiting the British Islands, except those which feed exclusively on other insects, may, under certain circumstances, or when unusually abundant, become entitled to a place in the category of injurious insects. Thus, the present Report opens with figures and descriptions of three species of *Lepidoptera*—the Eyed Hawk Moth, the Lappet Moth, and the large Tortoiseshell Butterfly—which are not generally so common in England as to be thought capable of causing serious injury; indeed, Miss Ormerod herself suggests that such pests might easily be got rid of by inviting an entomologist to clear them off. One of the most important insect visitations during the past year was that of the Antler Moth (*Charæas graminis*) in South Scotland, where the larvæ fed on the tender grass which sprang up over the districts previously devastated by the vole plague. Several pages are devoted to hay mites, which appear, however, though sometimes excessively abundant, to cause little real damage. Other interesting creatures noticed are the eel-worms (in her account of which Miss Ormerod has unintentionally in part anticipated Prof. Percival's important paper on the subject in *Natural Science* for March, as fully explained in an accompanying note), horse-warble, winter gnats, wasps, &c. It does not appear to be quite certain whether the horse-warble is the same species as the ox-warble. Warble in the horse appears to be of much less common occurrence than in the ox, and usually to occur only in small numbers in the same animal. But this may be due to the horse being a more carefully-tended animal than the ox. Two other points of interest deserve notice. Several species of carnivorous ground-beetles are stated to be very destructive to strawberries. More information is certainly required as to how far so-called carnivorous insects will also eat vegetable matter, decaying or otherwise. It appears that sea-gulls are sometimes in the habit of frequenting turnip-fields infested with the Diamond Back Moth, and in one instance a farmer attributed the destruction of his crop to the gulls. So easy is it to make a serious error in discriminating between the farmer's friends and foes.

A NEW and particularly convenient method of preparing the unsaturated hydrocarbon allylene, C_3H_4, has been discovered by Prof. Keiser and Miss M. B. Breed, as the result of an investigation concerning the action of magnesium upon the vapours of the alcohols. Preliminary experiments showed that when magnesium is heated with organic compounds such as alcohols, acids, and ketones, a reaction of considerable energy occurs at more or less elevated temperatures, accompanied by incandescence of the metal. The reactions between magnesium and the alcohols were then systematically studied. The magnesium, in the form of filings, was placed in a porcelain boat, and heated in a combustion tube through which the vapour of the alcohol was conducted. At a low red-heat the magnesium usually commenced to glow at one end of the boat, and the incandescence was then rapidly communicated to the whole quantity of metal, while large volumes of gas were evolved from the tube. When methyl alcohol is employed the action is extremely energetic, and the gases evolved consist mainly of about four-fifths hydrogen and one-fifth marsh gas. The residue in the boat is then allowed to cool in the vapour of the alcohol, when it is found to have the appearance of a black coherent mass. When this solid residue is placed in water a gas is slowly evolved, and if a few drops of ammonium chloride are added, the gas is liberated in a steady and moderately rapid current. The odour of the gas resembles that of acetylene, but when it is conducted through an ammoniacal solution of cuprous chloride the greenish yellow precipitate of cuprous allylide is produced, and with an ammoniacal silver nitrate solution the white crystalline precipitate of silver allylide is formed. These precipitates are highly explosive after separation and drying, a temperature of 150° being sufficient to bring about their explosion. When they are treated with dilute nitric or hydrochloric acids they dissolve with evolution of allylene. Ethyl alcohol behaves similarly towards heated magnesium, and the black residue left in the boat after the completion of the reaction is similarly decomposed by water with liberation of allylene, the rapidity of evolution of the latter being also largely augmented by the addition of a little ammonium chloride. The most convenient alcohol to employ, however, is propyl alcohol, for the black magnesium residue is in this case decomposed much more rapidly by water at the ordinary temperature, and the yield of gas is considerably larger. Prof. Keiser states that this method of preparing allylene for lecture purposes, by the reaction between magnesium and propyl alcohol, and subsequent decomposition of the product by water, is far preferable to the ordinary method of decomposing propylene bromide with alcoholic potash. Most of the other alcohols likewise afford magnesium residues which liberate allylene when brought in contact with water, but the gas is rarely so pure as that derived from the use of propyl alcohol.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*, ♂) from India, presented by Mrs. Turner-Turner; an Azara's Fox (*Canis azaræ*) from Brazil, presented by Messrs. Edgar and Harold Turner; four Amadavade Finches (*Estrelda amandava*) from India, presented by Mrs. Faulkenor; a Chukar Partridge (*Caccabis chukar*) from India, deposited; a Sykes's Monkey (*Cercopithecus albigularis*, ♀) from East Africa, two Red-crested Pochards (*Fuligula rufina*, ♂ ♀) from India, purchased; a Great Kangaroo (*Macropus giganteus*, ♂), three Hunter's Spiny Mice (*Acomys hunteri*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

SPECTRUM OF THE ORION NEBULA.—A full account of the photographs of the spectrum of the Orion Nebula, which were taken with Mr. Lockyer's 30-inch reflector at Westgate-on-Sea in 1890, has just been published in the *Philosophical Transactions* (vol. 186 A, p. 73.) Four hydrogen lines more refrangible than K are shown on the photographs, and in all 54 lines have been recorded. The line near wave-length 3730, first discovered by Dr. Huggins, is a very strong line, and among other prominent lines, is the well-known chromospheric line at wave-length 4471. It is shown that many of the principal lines are coincident with bright lines photographed in the spectrum of P Cygni at South Kensington, and with bright lines in planetary nebulæ and bright line-stars photographed by Campbell and Pickering, so that a close connection of these bodies is

established. Other tables show a close relationship between the bright lines of the nebula and the dark lines in the so-called Orion stars, of which Rigel and Bellatrix are typical examples.

The following are the conclusions to which the investigation has led: (1) The spectrum of the nebula of Orion is a compound one, consisting of hydrogen lines, low temperature, metallic lines and flutings, and high temperature lines. The mean temperature, however, is relatively low. (2) The spectrum is different in different parts of the nebula. (3) The spectrum bears a striking resemblance to that of the planetary nebulæ and bright-line stars. (4) The suggestion, therefore, that these are bodies which must be associated in any valid scheme of classification is strengthened. (5) Many of the lines which appear bright in the spectrum of the nebula, appear dark in the spectra of stars of Groups II. and III., and in the earlier stars of Group IV.; a gradual change from bright to dark lines has been found. (6) The view, therefore, that bright-line stars occupy an intermediate position between nebulæ and stars of Group III. is greatly strengthened by these researches.

THE ECLIPSE OF THE MOON.—The earlier phases of the total eclipse of the moon on Monday morning were observed under very favourable circumstances in the neighbourhood of London. The penumbra was not distinctly visible until about ten minutes before contact with the shadow, and the whole disc of the moon remained clearly visible, even at the middle of totality, until clouds stopped observations about 4 a.m. The parts deeply immersed in shadow were intensely red throughout, and with the telescope all the principal formations could be easily distinguished. During totality the sky was exceptionally clear, and numerous occultations were observed without difficulty. For half an hour after the commencement of totality the following edge of the moon was pretty brightly illuminated, and presented a striking contrast with the redness of the advancing edge; at mid-eclipse, however, the whole of the disc was very red.

It is reported that nearly 140 observations of disappearances or reappearances of eighteen stars were secured at the Royal Observatory on Monday morning by the eleven observers who watched the progress of the eclipse.

THE NAUTICAL ALMANAC, 1898.—In the recently published volume of the British Ephemeris for 1898, we note several valuable additions. The places of eleven close circumpolar stars, four of which are in the northern hemisphere, are given for each day of the year, and the mean places of fifty-two additional stars for navigational purposes have been added. The improved explanations of the contents will also no doubt be generally appreciated. The publication of an abridged edition for the use of seamen is a step in the right direction.

PHYSICAL WORK OF HERMANN VON HELMHOLTZ.[1]

I

THE career we are to consider this evening was a career of singular distinction. In days when the range of "natural knowledge" is so vast that most workers are compelled to be content if they can add something to one or two of the subdivisions of one of the main branches of science, von Helmholtz showed us that it is not impossible to be at once a great mathematician, a great experimental physicist, and, in the widest sense of the term, a great biologist.

It was but eight months yesterday since he delivered his last lecture; it is six months to-day since he died, and the interval is too short for us to attempt to decide on the exact place which will be assigned to him by posterity; but making all allowance for the fact that each age is apt to place its own great among the greatest, making all allowance for the spell which his name cast over many of us in the lecture-rooms where we ourselves first gained some knowledge of science, I am sure that I only express the views of all those who know his work best, when I say that we place him in the very front rank of those who have led the great scientific movement of our time. This opinion I have now to justify. I must try to convey to you in some sixty minutes an outline of the work of more than fifty strenuous years, to give you some idea of the wide range of the multifold activities which were crowded into them, of the marvellous insight with which the most diverse problems were attacked and solved, and, if it may be, some image of the man himself. The task is impossible, and I can but attempt some fragments of it.

The history of von Helmholtz is in one respect a simple tale. There are no life and death struggles with fate to record. His work was not done with the wolf at the door, or while he himself was wrestling with disease. He passed through no crises in which success or failure, immortality or oblivion, seemed to depend on the casting of a die. He suffered neither from poverty nor riches. He was a hale strong man on whom external circumstances neither imposed exceptional disabilities, nor conferred exceptional advantages, but who, by sheer force of the genius that was in him, passed on from success to success till he was recognised by all as the admirable Crichton of modern science, the most widely cultivated of all students of nature, the acknowledged leader of German science, and one of the first scientific men in the world.

It is the more fitting that this evening should have been set aside for the consideration of the work of Helmholtz, in that England may claim some share in his greatness. Before her marriage his mother bore an English name—Caroline Penn; she was, as her name implied, of English descent. His father was a Professor of Literature in the Gymnasium at Potsdam, so that his early days were passed amid that plain living and high thinking which are characteristic of intellectual circles in Germany. The boy did well at school, and when the time came for choosing a profession, his passion for mathematics and physics had already developed itself. The course of his love for these sciences did not run quite smooth. The path of his ambition was crossed by the hard necessity which in some cases checks, in others fosters, but in all chastens the aspirations of youth. He had to make his livelihood. Science must be to him what the Germans happily call a "bread-study." Medicine offered a fair prospect of prosperity. Physics, in those days, was but an intellectual pastime. And so the young man took his father's advice, and became an army doctor. In this, as in so many other cases, "the path of duty was the way to glory."

It is possible that if von Helmholtz had been what—with a sad consciousness of the limitations it implies—I may call a mere physicist, he would have played a greater part in the development of some of those subjects, the study of which he initiated or helped to initiate, but did not thereafter pursue. It is possible that had he been a biologist, and nothing more, he would have followed up the early investigation in which he disproved the old theory that putrefaction and fermentation are chemical processes only, clearly indicating, if he did not actually demonstrate, that the decay which follows death is due to an outburst of low forms of life.

He might thus under other circumstances have done work for which he showed his competence, but which is now chiefly associated with other names; but it is certain that without the unusual combination of wonderful mathematical power and a professional knowledge of anatomy, he would never have accomplished the special tasks which it is his special glory to have achieved.

His first three papers, however, hardly displayed the fusion between his various powers which was afterwards so remarkable a characteristic of his work. The first two were on biological subjects. The third was the famous essay on the "Conservation of Force." I have told elsewhere the story of the dramatic circumstances under which it was given to the world, of the interest it excited among the members of the Physical Society of Berlin, the refusal of the editor of *Poggendorff's Annalen* to publish it, and the final triumph of the author and his views. (*Fortnightly Review*, November 1894.) Helmholtz was not, and did not claim to be an original author of the doctrine of the conservation of energy; but two young men, Sir William Thomson in England, and Helmholtz in Germany, independently, and within a month of each other, were the first persons who compelled the scientific world to regard it seriously.

There is one interesting fact which connects this essay directly with the Royal Institution. Four years after it was published, it was placed by Du Bois Reymond in the hands of one who was lost to science in the same year as von Helmholtz himself—the late Prof. Tyndall. He was much impressed, and has spoken of the incident as bringing him face to face with the great doctrine of the "Conservation of Energy." ("Introduction to Popular Lectures by Helmholtz," translated by E. Atkinson, 1873.) He translated the essay into English, and

[1] A discourse delivered at the Royal Institution, by Prof. A. W. Rücker, F.R.S., on Friday, March 8.

for many years made it his habit to place every physical paper published by Helmholtz within the reach of English readers.

And now, having brought you to the point at which Helmholtz may be said to have been fairly started on his life's work, let me first briefly describe his official career, before I consider his work in greater detail.

When his extraordinary abilities became evident, he was permitted to sever his connection with the army. At twenty-seven years of age he became Teacher of Anatomy in the Academy of Arts at Berlin. In the next year he was appointed Professor of Anatomy and Physiology at Königsberg, and he held similar posts in the Universities of Bonn (1855–58) and Heidelberg (1858–71). It was not till 1871 that his early love for physics was finally rewarded. When the chair of Physics was to be filled in the University of the newly-founded German Empire, in Berlin, it was felt that even in Germany—the land of specialists—no better occupant could be found than one who was then in his fiftieth year, and had been all his life a teacher of anatomy and physiology. The choice was universally approved and completely justified, and von Helmholtz held this post till his death.

In this connection I am, by the kindness of Sir Henry Roscoe, enabled to show to you a relic of remarkable interest. It is a photograph of the great teacher and investigator, taken at the very last lecture that he delivered—that, namely, on July 7, 1894.

For some years, that is, from the date of its foundation, von Helmholtz was the president of the Physikalisch-Technische Reichs-Anstalt in Charlottenburg. This institution, founded partly by the munificence of the late Dr. Werner Siemens, partly by funds supplied by the State, has no precise analogue in this country. It is devoted to the carrying out of systematic researches on questions of fundamental importance to which a long time must be devoted.

The most characteristic work of Helmholtz was, as I have already hinted, that in which his knowledge of physics and his knowledge of anatomy were both directed to a common end. He dealt in turns with the external physical phenomena, with the mechanism of the organs which the phenomena affect, with the relations between the mechanical effect on the organ and the sensations which it excites, and, lastly, with the connection between the sensations in those simple cases which can alone be investigated in the laboratory, and the complex laws of æsthetics and art.

The two books in which these problems were chiefly treated were the "Physiological Optics," and the "Sensations of Sound." It is impossible to do more than lay before you a sample which may afford some idea of the intricacy of the problems with which he dealt, and of the pitfalls amongst which he walked so warily. For this purpose I have chosen one branch of his work on "Sound."

I have deliberately selected that particular portion which has been most questioned, that on which the verdict of most of those who have sat in judgment on his views has been against him.

In discussing this question I must give a general description of the principal phenomena; but if I were to attempt an exhaustive catalogue of all the facts disputed and undisputed, and of all the theories which have been based upon or upset by them, not only would time fail me, but those who have not given special attention to the subject would, I fear, become hopelessly confused amid the chaos of opposing statements and views. Another reason which urges me to be brief, is that a few years ago Prof. Silvanus Thompson explained the whole subject to the members of the Royal Institution, having kindly consented to act as the mouthpiece of the celebrated instrument maker, König, who has played so large a part in these controversies.

Among the chief achievements of Helmholtz was an explanation of the physical difference between pairs of notes which we recognise as concords and discords respectively. When two neighbouring notes are sounded, alternate swellings and fallings off of the intensity are heard, which are called beats. These produce an unpleasant effect, which depends partly on their number, partly on the relative pitches of the beating notes. When two notes beat badly, they form an intolerable discord. When they become separated by a wider interval, the beats are so rapid that they cease to be unpleasant.

The sense of dissonance produced by many of these wider intervals, such as the seventh (4 : 7), requires further explanation. In general, the fundamental musical note is only the first and loudest of a series of so-called partials, whose vibration frequencies are 2, 3, 4, &c., times that of the fundamental, and the consonance and dissonance of two notes is shown to depend on the presence or absence of beats between important members of these series. Thus in the case of the seventh the frequencies of the octave of the lower note and that of the upper note would be in the proportion 8 : 7, which are sufficiently near to make the beats very prominent and disturbing.

In cases where the notes are pure, that is, are not accompanied by upper partials, the explanation of dissonance is based upon another phenomenon.

When two notes are sounded simultaneously a third tone is often perceived, the frequency of which is equal to the difference of their frequencies. The number of vibrations of this tone is equal to the number of beats, and as there has been controversy as to whether the beats when they become rapid can produce a note, and if so, whether this note is or is not the same thing as the difference tone, it is necessary to distinguish between the two. This distinction is to be found in the mode of their production; but for the moment it is sufficient to remember that they may be distinguishable, and to reserve for them two names, viz. the beat-note, and the first difference tone respectively.

Helmholtz drew attention to the fact that together with the difference tone there is also produced a note, the frequency of which is equal to the sum of those of the two primaries, and this he called the first summation-tone.

Together with these he believed that there existed summation and difference tones of higher orders, the whole series being included under the name of combination tones. Our sense of dissonance between pure notes was explained as dependent on beats produced by the combination tones.

Up to the time of Helmholtz it was generally thought that these tones were produced in the ear itself, and had no objective existence in the external air. They are thus often called subjective, but as that adjective is usually reserved for impressions produced in the brain itself, it is better to say that they were regarded as *ear-made*. Helmholtz himself gave a theory, which showed that it is probable that a membrane like the drum-skin of the ear, which is forced out of shape by pressure, and that bones, like those in the ear, which can rattle, would, if acted upon by two notes, manufacture by their own proper movements all the varied combinational tones which his theory postulated. He therefore believed that combinational tones were largely ear-made.

You will observe that his theory of discord is quite unaffected by the question whether the combination tones are or are not sometimes objective. Provided only they are produced at all, it is immaterial whether they are produced in the ear itself. Von Helmholtz admitted that the phenomena we observe are in most cases ear-made tones; but he also asserted that they were sometimes objective, and could set bodies tuned to vibrate with them in resonant motion. This latter statement has been denied with singular unanimity, sometimes, I think, without due regard to the limitations which Helmholtz himself placed on the conditions under which the objective character of the notes can be realised.

All ordinary calculations as to the production and mingling of different waves of sound are based upon the supposition that the displacements of the particles of air, or other body through which the sound is travelling, are very small. If this is so, the force which tends to restore each disturbed particle to its ordinary position of equilibrium is accurately proportional to the amount of the displacement.

In von Helmholtz' view, objective combination tones were in general produced when the disturbance was so great that this condition was no longer fulfilled. Violence is of the essence of the explanation. Hence the siren, where both sets of holes open into the same small wind-chest—the harmonium, in which two reeds alternately close and open slits in the same enclosure, are the instruments best suited to produce them. Of these the siren is the more efficient. Von Helmholtz convinced himself that the combination tones produced by the harmonium are for the most part ear-made. He expressly stated that "when the places in which the two tones are struck are entirely separate and have no mechanical connection, as, for example, if they come from two singers, two separate wind instruments, or two violins"—to which we may add two tuning-forks—"the reinforcement of the combinational

tones by resonators is small and dubious." ("Sensations of Tone," translated by Ellis, p. 157.)

Now this reinforcement by resonators has been altogether denied by most of those who have taken an interest in the matter, while, if an exception is allowed, it is in favour of the beats of a disturbed unison, the observed effects being ascribed to the beats, and not to the difference tone.

Some writers make no exception whatever in their denial of the objective reality of what may be broadly termed secondary tones. Thus Mr. Bosanquet, who made a most careful series of experiments some fourteen years ago, stated that "the ordinary first difference tone . . . is not capable of exciting a resonator. . . . In short, the difference tone of Helmholtz . . . as ordinarily heard, is not objective in its character." (*Proc. Phys. Soc.* iv. 1881, p. 233.)

Prof. Preyer, too, using very sensitive tuning-forks, found that the differential tone given by two forks did not affect a fork the frequency of which corresponded with its own, except in cases where the difference tone was itself a partial of one of the forks.

It must be remembered that the assertions of Helmholtz as to the experimental proof of the objective nature of the tones were made with reference to those instruments which he regarded as most likely to produce objective notes, viz. the siren and the harmonium, and that, therefore, experiments with forks hardly affect his position.

Let us now try with the siren whether it is possible to confirm or to disprove the validity of his views.

For this purpose the rather bulky apparatus which you see before you has been constructed. I should hardly have been able to realise the idea embodied in it, at all events in time to show it to you this evening, if I had not been favourably situated in two respects. In the first place, I have had the zealous co-operation of one of my assistants, Mr. Edwin Edser, who has not only made all the parts of the apparatus that required to be newly made, but has thrown himself into the investigation with the utmost energy, working at it late and early, and making many valuable suggestions and improvements. In our joint work we have been helped by some of my senior students, and notably by Messrs. Cullen and Forsyth. In the second place, I have had at my disposal the magnificent collection of acoustical apparatus in the National Museum at South Kensington, some of which I am allowed, by the kindness of the Department of Science and Art, to bring here this evening.

wave-length of light, the path of the ray which falls upon it is shortened by a whole wave-length, and the position of each band is shifted to that previously held by its neighbour. If the fork vibrates with an amplitude of this almost infinitesimal amount, the bands will disappear, or will alternately appear and disappear according to circumstances. The fork may therefore be used to detect by resonance the presence of vibrations, the frequency of which is 64 *per second*.

A priori, there were two difficulties of opposite kinds which made it doubtful whether the fork would be an efficient weapon for the purpose for which it was to be used.

In the first place it would feel tremors of any sort, and it was doubtful whether it would be possible to discriminate between mere shakes and the vibrations which were to be studied. This difficulty has been very largely overcome.

The table on which the apparatus stands rests on india-rubber. On the table are a pair of library steps; these support two pieces of wood, which are heavily weighted and rest on india-rubber balls. From these two beams hang steel wires, which carry india-rubber door-fasteners, and these in turn support two rods on which the paving-stone is placed. By this alternation of elastic and of heavy bodies we can make the bands absolutely steady, unless the disturbances are violent. The quiet movements necessary for working the apparatus, the blowing of the bellows, and the like, produce no effect. On the other hand, the shutting of a door in a distant part of the building, the rumble of a cart in the street, will cause the bands to disappear. A great deal of the work on which we rely has been done at South Kensington between midnight and three o'clock in the morning. Trustworthy observations have indeed been made at other times, but it is only in the still small hours that the apparatus is at its best.

The second doubt was of a different kind. It was certain that the instrument would be more or less shaken; it was not quite certain whether the fork would respond to vibrations of the given period. It is easy to set a tuning-fork in vibration by resonance when it is mounted on a sounding box, but in that case the vibrations of the enclosed mass of air are communicated through the box to the fork. When the stalk of the fork is held rigidly, a tuning-fork is notoriously difficult to excite by resonance. This objection is, of course, to some extent counterbalanced by the extraordinary sensitiveness of the means of detecting the vibrations, but it is necessary to supplement this by other devices. The instrument used is a siren (S). In front of it is placed a hollow wooden pyramid, the narrow end of

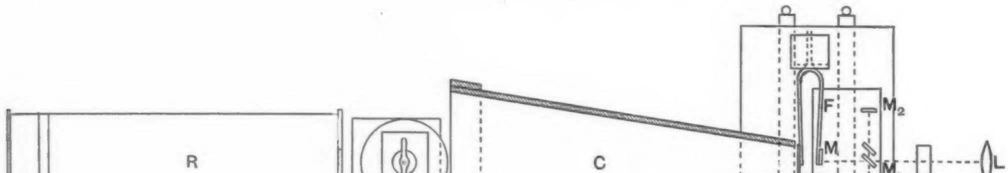

FIG. 1.

The essential part of the apparatus, Fig. 1, is a tuning-fork, F, to one prong of which is attached a mirror, M, and to the other a square of thin wood, strengthened by ribs, which is of the same weight as the mirror. The fork thus loaded has been compared with one of König's large standards by means of Lissajous' figures. Its frequency does not differ from 64 complete vibrations per second by more than one vibration in two minutes. The shank is supported by a mass of lead, which in turn is placed upon a paving-stone. Upon this stone also rest the other mirrors necessary for producing Michelson's interference bands. The mirror, M_1, is silvered so thinly that half the light which falls upon it is reflected, and half is transmitted.

A ray proceeding from the lantern, L, will be divided at M_1, into two, which follow the paths $LM_1M_2M_1B$ and LM_1MM_1B respectively. Interference bands are thus produced, which can be projected on to a screen, so as to be rendered visible to a large audience.

If the prong of the tuning-fork moves through the eighty-thousandth of an inch, that is, through a distance equal to a half

which is near to, and is of the same area as the wooden plate attached to the tuning fork. This serves to collect the waves of sound, and to concentrate them on the fork. Behind the siren is a large resonator by König, timed to respond to 64 vibrations per second.

In some respects the apparatus requires careful handling. Of course if you blow down the collecting cone the fork may be disturbed, and sometimes a particular note of the siren appears to affect the fork for no very obvious reason. Probably the resonance of the air in the cone, or the vibrations of the wooden disk, may at times be the causes of such effects. We have, however, found that whatever they may be due to, they differ in appearance from those produced by vibrations synchronous with the periodic time of the fork, and they can in general be got rid of by a very slight readjustment of the apparatus. The fact that our main conclusions do not depend on any such nicety, is proved by the fact that the instrument has been set up twice in the laboratory, and once in the lecture-room in the College. In

each case all the experiments have been successful, and on one occasion only were we troubled by a disturbance due to a note (of about 253 vibrations) when sounded alone. A slight readjustment of the cone, however, eliminated this effect entirely.

Such difficulties make it no easy matter to set up the apparatus in a hurry, and the most I can hope to do this evening is to demonstrate to you the methods of using it. I cannot undertake to make the actual measurements before you.

It is, however, desirable to illustrate the sensitiveness of the apparatus to vibrations of 64 per second, and its insensitiveness to other sounds.

Provided the current of air does not travel directly down the cone, organ-pipes may be blown just outside it without producing any effect. One of Konig's large tuning-forks may be bowed strongly without effect.

If, however, the exciting fork be tuned to 64 vibrations per second, and if it be struck as lightly as possible with the handle of a small gimlet, used as a hammer, the handle having been previously covered with india-rubber, the bands will immediately vanish, though the note produced is often quite inaudible, even to a person whose ear is placed close to the fork.

Let the weights on the fork be shifted so that it makes 63·5 vibrations per second, then the resonating fork beats, and the bands regularly appear and disappear every two seconds.

Having thus explained the construction and working of the apparatus, let me show you how we have tested whether it responds to a difference tone. When the proper rows of holes are opened, the siren will give simultaneously the c' of 256 and the e' of 320 vibrations. The interval is a major third, the difference tone is 64 vibrations. The pitch is determined by the beats between the upper note and a standard tuning-fork which gives e'. Sounding the upper note alone no effect is produced on the interference bands, as the beats first appear, then die out, and are finally heard again when the note given by the siren is too high.

It could be shown in like manner that the 256 note alone produces no effect, but if, when the standard fork of 320 vibrations and the upper note of the siren are judged to be in exact accord, the 256 note be also produced, the bands immediately disappear. Sometimes, of course, a small error is made in the estimate of the pitch, and the effect is not instantaneous, but in every case the bands disappear when the beats between the two notes are so slow that they cannot be distinguished.

It is therefore evident that Helmholtz was right when he asserted that the difference tone given by the siren is objective. It exists outside the ear, for it can move a tuning-fork.

(To be continued.)

JAMES WATT AND OCEAN NAVIGATION.[1]

IF it be asked what James Watt did during his long, busy, and eventful life to improve ocean navigation, or to adapt the steam engine to the work of propelling ships, I am obliged to reply that I am not aware he personally did anything, or even that he concerned himself much about the matter. He took no active part that we know of in applying or adapting his steam engine to the propulsion of ships. The reason probably was that after his attention was first directed to the subject of the steam engine, or fire engine, in 1759, his whole energy was expended, first in improving the steam engine and making its manufacture commercially successful, and afterwards in executing the orders that came for pumping and other engines that were required for mines and manufactures. In the case of most of the greatest mechanical inventions—Watt's among the number—it has not been the ideas or the inventions by themselves that have brought success, prosperity, or even satisfaction to their owners. These results have had to be painfully and slowly evolved out of long and costly practical demonstrations and experience of the alleged merits of the invention. James Watt toiled, suffered and endured for more than twenty years after his discovery of separate condensation in 1765, before he could see that his steam engine would ever bring him anything but disappointment, loss, and misery. It is highly characteristic, however, of Watt's fertile and original genius, and significant of what he might have done to develop the marine engine at the commencement of its history, had he taken the matter up, that upon the two principal occasions we know of when he applied his mind to the subject, he made very pregnant suggestions. Thus, when Watt sent drawings of his engines to Soho in 1770 for Mr. Boulton to construct one for experiment, and had been told that it was intended to make an engine to draw canal boats, Watt wrote, "Have you ever considered a spiral oar for that purpose, or are you for two wheels?" and to make his meaning clear he sketched a rough but graphic outline of a screw propeller. This is, perhaps, the earliest suggestion of a screw propeller, except that it was proposed by Daniel Bernoulli, the mathematician, in 1752. Again, in 1816, four years after the first Clyde steamboat, the *Comet*, was built at Port Glasgow, when Mr. Watt was upon his last visit to Greenock, he went to Rothesay and back in a steamboat. At that time the engineer did not reverse his engines, but merely stopped them some time before the vessel reached her mooring-place, and let her gradually slow down. James Watt, then an old man of eighty, tackled the engineer of the boat, and showed him how the engine could be reversed. He tried to explain this with the aid of a foot rule, but not being successful in doing it to the complete satisfaction of the engineer, he is said to have thrown off his overcoat and given a practical demonstration. Although Watt never took up the subject of steam navigation and never made a marine engine, still he was in reality its originator, because he discovered and provided the means by which it could be applied with advantage to the propulsion of ships. Each of his great improvements upon the old engine that worked by atmospheric pressure and condensed its steam in the cylinder—such as the separate condenser, the working by steam pressure as well as by pressure obtained by vacuum, the double action of the steam in the cylinder on both sides of the piston, working the steam expansively, the centrifugal governor for automatically regulating the speed of the engine, and many others—was a direct adaptation for marine purposes.

There is one point in the history of shipping at which we can draw a definite line between old and new when changes were made so radical in their nature, and so rapid and universal in their operations, that all which came after is fundamentally different from what existed before. The period of transition falls in the early part of the present century, when the propulsion of ships by steam power was substituted for propulsion by the wind—the motive power that had been employed from time immemorial—and when the material out of which their hulls were built was changed from wood to iron. The lateness of this period and its near proximity to the present, is illustrated by the fact that it was not till after the accession of H.M. Queen Victoria that steamships and ships built of iron came to be regularly employed in ocean navigation. At the close of the first third of the nineteenth century, the over-sea trade of the world was carried on with ships that were all built of wood and propelled by sails. Only about 200 of these were over 500 tons in burden, or much over 100 feet long. Nothing approaching to such a rapid and complete revolution as these two great changes brought about in the dimensions, forms, and all the characteristics and qualities of ships, in the conditions of life on board ship, and in travelling by sea, was ever experienced before in the known history of shipping. All the old ships of which we have any knowledge—and by old ships I mean all that existed prior to the introduction of steam—were built and fashioned entirely by manual power, with the aid of very simple tools; and they were either propelled through the water by manual labour, or by sails that could be worked in the simplest manner by the crew. One of the broadest distinctions between the ships of the past that were built of wood and propelled by sails and those of the present that are built of iron or steel and propelled by steam, is that everything had to be done in the former by the hand of man, without any aid from machine tools or other modern labour-saving and labour-helping appliances. And this was so both in preparing the materials used in building the hull and shaping them to their requisite form, putting them in position, fastening them together, and in working the ship at sea and handling the sails so as to make the pressure of the wind most effective for propulsion. In modern ships, almost everything is, on the other hand, done by steam-power in its various applications. It is by this means the plates which form the hull are first of all rolled

[1] Abstract of the Watt Lecture, delivered by Dr. Francis Elgar at Greenock, on January 18.

and are afterwards cut, drilled, bent to the required form, and many of them riveted; and it is by steam-power also that ships, after they are built, are propelled through the water, steered, pumped and drained, ventilated, lighted, loaded and discharged; the anchor is weighed, guns are trained, loaded and fired, and all the principal working operations are carried on. There could have been no great difference in size between the ships of 2000 years ago and the trading vessels of the last century. It is the application of steam-power to propulsion and to manufacture that has enabled vessels to be produced the dimensions and proportions of which were formerly unapproachable. The employment of iron and steel as the material of construction would have been impossible without the aid of steam-power; and it is the extra strength of hull obtained by these means which enables ships to be built of the large size that has now become common. Steam-power has thus not only furnished a mode of propulsion certain and regular in its action, and enabled ships to make their voyages with little or no regard to wind or weather, but it has, in manufacturing the raw material out of which ships are built, permitted the dimensions to be very largely increased, and that not only without risk and inconvenience, but with very great increase of accommodation, comfort, and safety. It is sometimes thought that the largest ships are essentially more unsafe than those of smaller size; the fact is, increase of size enables a vessel not only to be made easier in her movements at sea and less affected by the waves across which she is travelling, but it also enables the largest ships to be divided into so many separate water-tight compartments as to be practically unsinkable by the action of the heaviest seas, or by the worst effects of a collision. I do not say that all large ships are constructed so as to possess this high degree of safety, but many of the latest ones are, and it is perfectly practicable to obtain in cases where safety is the principal consideration. In small vessels the same degree of safety could not always be obtained. Safety is a quality that can be much increased by growth in dimensions.

Although James Watt may not have helped actively in the application of steam-power to ships, it is really to him and his inventions we have to look as the source whence all the great modern improvements in ocean navigation have been derived. We find in James Watt the typical engineer. He was a great philosopher and a great mechanic. He possessed just the combination of qualities and the temperament requisite to enable a man to ascertain what may be learned of the forces of nature and their mode of operation, and to utilise and apply these in the most direct way for producing a required result. He formed that happy union of what is commonly called the "theoretical" with the "practical" man. For as there was no better practical mechanic than Watt in the country, so was there no more diligent student of the sciences related to the subjects of his work, or a more patient and thorough investigator of the principles or theories upon which it depended. He tested everything by experiment; and it is said that when asked an opinion of a novel invention or proposal, his reply invariably was, "Make a model." But having ascertained by experiment all he could learn of the facts connected with any subject he was investigating, he was never satisfied till these could be explained by some physical law with which they could be shown to accord. His mental attitude towards the great mechanical problems he took in hand was that of one engaged in a close and desperate struggle with nature herself, questioning, cross-examining, testing by experiments, attacking from all sides, and refusing to give in till he had succeeded in discovering the particular secret he required to know. A favourite saying of his was, "Nature can be conquered, if we can but find out her weak side."

We thus see what are the qualities necessary to make a great engineer. They are mechanical skill and experience, scientific knowledge and capacity, great powers of observation and original investigation, energy, patience, and untiring perseverance. There have been great engineers who have exhibited certain of these qualities in a very high degree, but none who possessed all together in such full measure and such harmonious blending as we see in the case of Watt. No one man could otherwise, in a few years, have transformed so rude and imperfect a machine as the steam-engine was when Watt first took hold of it into the most perfect instrument that the working capabilities of the time admitted. The proof of Watt's great power as a mechanic and philosopher combined are to be found in the fact that he perfected in such a short time, within the limitations that were imposed by the quality of the materials and the workmen of the day, the greatest work that has been performed by any engineer of modern times.

We often hear the question asked by anxious parents or aspiring youths: How can my son, or how can I, as the case may be, become an engineer or a naval architect? This is sometimes asked as though the making of an engineer or a naval architect were perhaps a matter of three or four years' work in an office, combined with a certain amount of study of books, or attendance at lectures. There are few persons not belonging to one of the many branches of the engineering profession who know what this question really means. Engineering—and when I say engineering, I include in the term shipbuilding and all other branches of that grand profession, "whereby the great sources of power in nature are converted, adapted, and applied for the use and convenience of man"—engineering has of late become somewhat fashionable, and has attracted the notice of classes in the community who at one time would have despised it as a base, mechanic art, and turned their backs upon it. It has apparently acquired the reputation of being a well-paid profession and of being worth belonging to, in a money sense. To the general body of inquirers who thus look to engineering as offering better financial prospects than the army or navy, than the law, medicine, or the Church—including some who think there might be a chance in that direction after failing to qualify for one of the professions named—let me say that the prospects of success are very remote unless he who enters upon it is gifted with mechanical aptitude and skill, is willing to gain experience by a long course of hard work, and at the same time has the capacity, the taste, and the time for acquiring a sound knowledge of the mathematics and the physical sciences that relate to the particular branch of engineering he may think of taking up. Competition is now very keen in all departments of engineering, and what prizes there may be to strive for in them, can only fall to those who are exceptionally gifted with knowledge, experience, energy, and determination. An ordinary student or apprentice who can only learn what some one teaches him, and has not much faculty for independent observation, or for reflecting upon and discovering the causes of the many things he sees going on all around him, is never likely to be more than a subordinate in the ranks of the profession, a hewer of wood and a drawer of water, for others who have greater power of acquiring knowledge, of thinking for themselves, of observing and investigating closely and accurately the causes of defects and difficulties that arise out of their work, and of devising the necessary means of overcoming them.

If poets "are born and not made," I am sure it is equally the case with a great engineer like James Watt. His wonderful mechanical skill and ingenuity were natural to him, and were the means of determining the course his life would take. But even with all that, it is quite clear he could never have made his great discoveries and improvements had he not been a naturally gifted and diligent student, and acquired all the knowledge that was obtainable at the time of mathematics and natural philosophy. It is true that scientific study alone cannot make an engineer; but with the example of such a great mechanic as James Watt before us, it would be very presumptuous for any to say of himself that his own practical knowledge and judgment are sufficient to make him a fully-qualified engineer without studying what others have said or done upon the subjects of his work, or the physical laws that underlie the whole fabric of his practice and ideas. I would be the last to encourage any young man to suppose that a short course of study, or even great progress in mathematics and the physical sciences, would justify him in thinking himself an engineer; but I am at the same time perfectly sure that no one, however great his mechanical skill and ability might be, could ever become an engineer in the true sense of the term without following Watt's example of studying, thinking, and diligently acquiring all the knowledge of nature and nature's laws he can obtain; and applying such knowledge to the better understanding of the principles which relate to his work, and upon which the degree of success he may achieve in it depends.

One other remark with regard to James Watt. I have spoken of the great benefits we have derived in this country from the application of his steam-engine to ocean navigation by drawing the various parts of the world closer towards us, and converting the sea into a broad highway that unites us to the different continents

and islands upon it in a neighbourhood which is becoming nearer and more intimate every year. We often speak complacently of the advantage this is to our own country in particular—of what it has done in enormously increasing the wealth and prosperity of the rich, and ameliorating and brightening the lives of the poor—in promoting the growth of our manufacturing trades—in enabling food to be imported from abroad in large and regular supplies at much cheaper rates than we could produce it ourselves in these islands, and in the great increase of population that the growing prosperity of the country and the easier conditions of life have thus brought about. All this is true, and it represents an extent of change and of progress during a short space of time that we can only look and marvel at, as being due to so large an extent to the results of one man's inventions. But there are other feelings with which we do well to regard the matter besides those of wonder and admiration, and of self-satisfaction with the great prosperity and the numerous advantages the country has reaped. We have been favoured above most other peoples by all these great changes, and have been blessed in very bountiful measure. We must not forget, however, that among the privileges we thus enjoy, that of immunity from danger and harm is not included. There are few pleasures or privileges to be had without alloy; and we now find, as a set-off against the benefits obtained through the improvements in ocean navigation, that we have much greater responsibilities and difficulties in protecting ourselves against danger, and in preserving unimpaired for the future the heritage of power and prosperity that has been handed down to us. The same causes that make ocean navigation easy, swift, and certain for us, make it easier also for any possible enemies to attack us. The great increase of population, due to the recent growth of wealth and prosperity, requires for its existence constant supplies of raw material to be kept up from abroad, in order that our surplus hands may be profitably employed in manufactures, and it requires also large and continuous food supplies from outside in order that it may be fed. Hence the great problem of the time for this country—how to protect ourselves against the dangers and drawbacks of the new state of things, while enjoying for the time its advantages and reaping its rewards; and how to effectually shield the vulnerable points in our armour that have arisen out of changes and improvements which brought so much good in other ways. It is upon the sea that any real danger to England would arise; and upon the sea it would have to be met. Let us hope that the nation which has covered all the seas of the world with its ships will not now fail in energy and enterprise, or be slow in providing and maintaining adequate defence of what it has produced with such success, and out of which it has reaped such rich reward. If we were to fail thus in our duties, and so shirk our responsibilities, the improvements and benefits we owe so largely to the genius of James Watt might, after all, prove a curse instead of a blessing; and we should be unworthy of the country and the race which produced the great engineer who taught his contemporaries, more than one hundred years ago, how to manufacture Power.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—Mr. Francis Gotch, F.R.S., Holt Professor of Physiology in University College, Liverpool, has been elected to the Waynflete Professorship of Physiology, vacated by the appointment of Dr. Burdon Sanderson to be Regius Professor of Medicine. Prof. Gotch is no stranger to Oxford, having been for some years assistant to his predecessor in the Waynflete Chair.

CAMBRIDGE.—Dr. W. S. Lazarus-Barlow, of Downing College, has been appointed Demonstrator of Pathology in the room of Dr. J. Lorrain Smith, who has been elected Lecturer in Pathology at Queen's College, Belfast.

The Examination in Sanitary Science for the University Diploma in Public Health will begin on April 2.

Hitherto one of the conditions which had to be fulfilled before the Science and Art Department made payments to the Committees of schools or classes, for the instruction of students, has been that the parents of a student should not have an income exceeding £400 a year from all sources. A Blue-paper now informs us that the Lords of the Committee of Council of Education have decided to enlarge this limit to £500 per annum. In future, therefore, the student on account of whom a claim is made must belong to the category of "persons in the receipt of not more than £500 a year from all sources, that is, who are allowed an abatement of the income-tax; and their children if not gaining their own livelihood." This example could be followed with advantage by the Technical Education Committees of those County Councils that restrict their Scholarships to competitors whose parents are in receipt of less than £120 a year.

SCIENTIFIC SERIALS.

Bulletin de l'Académie Royale de Belgique, No. 1.—Is the declination indicated by a compass independent of its magnetic moment? by Ch. Lagrange. According to Gauss's theory it may be assumed that the magnetic axis of a magnet lies in the direction of the lines of force of the field, whatever its magnetic moment may be. But in practice it is found that the orientation of a magnet depends upon the strength of its magnetisation. Since these systematic differences are not due to magnetic force, they must be due to some other force, probably a force hitherto unknown. Hence the magnetic chart of the earth calculated by Gauss's theory cannot be considered rigidly correct. A new constant must be introduced, depending upon the declinometer. The author foreshadows an explanation of these facts, based upon the "circulation of the ether," and intimately associated with the physics of the globe.—Double decompositions of vapours, by Henryk Arctowsky. It is not necessary that two substances should be dissolved in water to bring about their mutual decomposition; or their "ionisation," in terms of the electrolytic theory, is not altogether dependent upon water. Freshly sublimated mercuric chloride and flowers of sulphur were placed in small vessels inside a Bohemian glass tube over an organic combustion furnace. A current of pure dry hydrogen was introduced, which on heating formed sulphuretted hydrogen with the sulphur. This gas and the vapour of $HgCl_2$ gave a precipitate of mercuric sulphide on the walls of the tube. This reaction, which is contrary to Berthelot's principle of maximum work, does not take more time than the corresponding reaction in water. To prove that it was a true double decomposition, CO_2 was substituted for the hydrogen, when it was found that the sulphur vapour alone was unable to attack the mercuric chloride.

Bulletin de l'Académie des Sciences de St. Pétersbourg, fifth series, vol. 1, No. 4, 1894.—Minutes of proceedings for October last.—On derived functions of superior orders, by N. Sonin (in Russian).—Crustacea Caspia: contributions to the knowledge of the Carcinological fauna of the Caspian Sea, by G. O. Sars (in English, with eight plates). The Gammarideæ are continued, and the following species, mostly new, are described and figured: *Gammarus Warpachowskyi*, *minutus*, *macrurus*, *compressus*, *similis*, *robustoides*, *crassus*, *abbreviatus*, and *obesus*, and *Niphargoides caspius*, Grimm.—On the transformation of Periodical Aggregates, mathematical paper by H. Gyldén (in German).—On Free Energy, by B. Galitzine (in Russian).

SOCIETIES AND ACADEMIES.

LONDON

Royal Society, January 24.—"Notes of an Inquiry into the Nature and Physiological Action of Black-damp, as met with in Podmore Colliery, Staffordshire, and Lilleshall Colliery, Shropshire." By Dr. John Haldane.

Black-damp, sometimes also called choke-damp, or "stythe," is one of the gases frequently found in the workings of coal mines. It is distinguished from fire-damp by the fact that it is not explosive when mixed with air, but extinguishes flame; and from after-damp by the fact that it is not the product of an explosion, but collects in the workings under ordinary conditions. Like after-damp and fire-damp, it produces fatal effects when inhaled in sufficient concentration. A further distinction has been drawn between black-damp and white-damp, which latter is described as capable of supporting combustion, while at the same time acting as a poison when inhaled.

The author has made a number of observations on concentrated black-damp from two pits, the first being in a fiery

and the second in a non-fiery district. The conclusions arrived at are as follows:

(1) The specimens of black-damp consisted when undiluted of nitrogen containing an admixture of a seventh to an eighth of its volume of carbonic acid.

(2) Air containing just sufficient black-damp to extinguish a candle or oil lamp produced no immediately sensible action on a man. A mixture of about 16 per cent. of the black-damp and 84 per cent. of air extinguished candles and lamps, whereas a mixture of about 60 per cent. of the black-damp and 40 per cent. of air would be required to produce immediate danger to life.

(3) The dangerous physiological action of black-damp is due to deficiency of oxygen, not to excess of carbonic acid. The effect first appreciable when increasing proportions of black-damp are breathed is, however, due to carbonic acid alone.

February 21.—"The Composition of the Extinctive Atmospheres produced by Flames." By Prof. Frank Clowes.

In a former paper (*Roy. Soc. Proc.*, vol. lvi.), the author communicated the results obtained by mingling gases, which were extinctive of flame, with air, until a flame burning in the air was just extinguished. The gases used in the experiments were carbon dioxide and nitrogen. Each of these gases was separately introduced into the air, and the composition of the atmosphere thus produced, which just extinguished flame, was determined by chemical analysis.

The general results arrived at were:—

(1) That wick-fed flames require atmospheres of very similar composition to extinguish them: while gas-fed flames require atmospheres of widely differing composition.

(2) That nitrogen must be added in larger proportion than carbon dioxide, in order to extinguish the same flame.

(3) That the minimum proportion of extinctive gas which must be mingled with air in order to extinguish a flame is independent of the size of the flame.

A supplementary series of experiments has now been undertaken in order to determine the composition of the atmosphere extinctive of each flame, which is produced by the flame itself when burning in an enclosed volume of air at atmospheric pressure. The apparatus used and the method of experimenting are fully described.

As in the previous series of experiments (*loc. cit.*) the combustible substances used were chiefly those which are burnt for ordinary heating and lighting purposes—namely, alcohol (absolute), alcohol (methylated), paraffin (lamp oil), colza and paraffin, candle, hydrogen, carbon monoxide, methane, and coal-gas.

Determinations were made of the percentage composition of the residual atmospheres left by the flames, and these were compared with the composition of the artificial atmosphere in which flame is just extinguished, and with the composition of atmospheres which are respirable according to the recent experiments of Dr. Haldane (*Proc. Roy. Soc.*).

The conclusions drawn from the tabulated results of the experiments published in the paper are that:—

(1) The flames of the combustible gases and liquids, which were experimented upon, produce, at the point of extinction in an enclosed atmosphere, a change in the proportion of oxygen in the air generally corresponding to that produced by preparing extinctive atmospheres by artificial mixture.

(2) The flames of candles and lamps, when they are extinguished by burning in a confined space of air, produce an atmosphere of almost identical composition with that of air expired from the lungs.

(3) The extinctive atmospheres produced by the combustion of the flames of candles and of lamps, and the air expired from the lungs after inspiring fresh air, are respirable with safety.

(4) The extinction of an ordinary candle or lamp flame is not necessarily indicative of the unsuitability of an atmosphere to maintain life when it is breathed.

Geological Society, February 6.—Dr. Henry Woodward, F.R.S., President, in the chair.—On bones of a Sauropodous Dinosaur from Madagascar, by R. Lydekker, F.R.S. The bones described in the paper were collected by Mr. Last to the east of the town of Narunda, on the north-eastern coast of Madagascar. They include vertebræ, limb-bones, and portions of pectoral and pelvic girdles. These bones were described in detail, and the animal which possessed them was referred to the genus *Bothriospondylus*, Owen; a dorsal vertebra, described in the paper, being taken as the type of the new species. The identification of the Malagasy reptile with a type occurring in the Jurassic rocks of England harmonises with the reference of some of the strata of the island to the Jurassic period.—On the physical conditions of the Mediterranean basin which have resulted in a community of some species of freshwater fishes in the Nile and the Jordan waters, by Prof. E. Hull, F.R.S. The author summarised the evidence in favour of the existence of barriers in post-Miocene times, separating the Mediterranean area into a chain of basins. He brought forward arguments in support of his contention that the waters of the eastern (Levantine) basin became fresh during a period when the area of evaporation was smaller, and the supply of river-water greater, than at present. Into this freshwater lake the waters of the Nile would flow directly. He had elsewhere given reasons for believing that the Jordan Valley from Lake Huleh to Arabah was the bed of a lake over 200 miles long, and at least 1300 feet above the present level of the Dead Sea. He suggested that the waters of this lake escaped into the Levantine basin through the plain of Esdraelon. With such physical conditions existing, the fauna of the Levantine basin would have had a means of spreading throughout the whole system of waterways connected with it. In conclusion the author added some observations on the changes which occurred in the Mediterranean area subsequent to the post-Miocene epoch of earth-movement.—On the loess and other superficial deposits of Shantung (Northern China), by S. B. J. Skertchly and T. W. Kingsmill. The following deposits were described in the order of their antiquity:—(1) Recent fluviatile deposits. (2) Marine sands with *Cardium*, *Ostræa*, and *Bulla*, extending to a height of 200 feet above sea level, and indicating former submergence to that amount. (3) Old river gravels, often resting on loess, and possibly contemporaneous with the marine gravels. They furnish part of the evidence relied on by the authors for supposing the existence at that time of a climate moister than the present one. (4) Loess. (5) Basement-gravels having the same relation to the loess that the Upper Greensand bears to the Chalk. The loess east of the Pamirs is extensively developed over an area of over one million square miles. It is sometimes over 2000 feet thick, and occurs up to several thousand feet above sea-level. Evidence was brought forward by the authors with the intention of establishing the absolute want of connection between the Chinese loess and the present river-systems, its original stratified condition (as shown by variation of tint and horizontality of layers of concretions), and its subsequent rearrangement to a great extent. The absence of marine shells was discussed, and the suggestion thrown out that the shells had been destroyed by percolating water. The authors gave their reasons for supposing that the loess is a marine formation, and stated that the sea need not have reached to a higher level than 600 feet above the present sea-level, for the Pamir region where it occurs, 7000 feet above the sea, is an area of special uplift. They maintained that there are no proofs of the glaciation of Northern and Eastern Asia, so that Chinese loess could have no connection with an area of glaciation. They stated that the zoological, ethnological, historical, and traditionary evidence alike pointed to the former depresssion of Asia beneath the sea, and the subsequent desiccation of the land, consequent upon re-elevation.

Mathematical Society, February 14.—Mr. A. B. Kempe, F.R.S., Vice-President, in the chair.—The chairman announced the decease, since the January meeting, of Prof. Cayley, F.R.S., and Sir J. Cockle, F.R.S., and stated that the Society had been represented at the funeral of the former by the President, himself, and Profs. Elliott, F.R.S., and Henrici, F.R.S. Messrs. Walker, F.R.S., Glaisher, F.R.S., and Elliott, F.R.S., paid tributes to the memory of the deceased gentlemen, and a resolution was passed unanimously that the President (Major Macmahon, F.R.S., who was absent owing to a domestic affliction) be requested to convey, in such form as he should think fit, votes of condolence from the Society to Mrs. Cayley and Lady Cockle.—Dr. Hobson, F.R.S., gave a brief sketch of a paper, by Mr. H. M. Macdonald, on the electrification of a circular disc in any field of force symmetrical with respect to its plane.—Prof. Elliott read a paper on certain differential operators, and their use to form a complete system of seminvariants of any degree, or any weight.—Prof. W. Burnside, F.R.S., sent notes on the theory of groups of finite order, iii. and iv. Herr Hölder, in a paper in the *Math. Ann.* vol xl., and Dr. Cole, in a paper in the *American Journal of Mathematics*, vol. xv., have

determined all the simple groups whose orders do not exceed 660. The only other known results in connection with the question as to whether there is a simple group corresponding to a given order are as follows. There is no simple group whose order is the power of a prime, or whose order contains two or three prime factors; and the only simple group whose order contains four prime factors is a group of order 60. The latter result was established in a paper published in vol. xxv. of the *Proceedings* of the Mathematical Society. The present paper aims at an extension of these results. If $p_1, p_2, \ldots p_n$ are distinct primes in ascending order, it is shown that in a group of order

$$p_1 p_2 \ldots p_{n-1} p_n^m$$

the number of operations whose orders are divisible by p_s and by no lower prime is

$$(p_s - 1) p_{s+1} \ldots p_{n-1} p_n^m,$$

from which it follows at once that a group whose order is of this form can neither be simple nor contain a simple sub-group.[1] It is also shown that a group whose order is of the form

$$p_1^2 p_2 \ldots p_{n-1} p_n^m$$

cannot be simple, and cannot contain a simple sub-group, with the exception of the case in which it contains a tetrahedral sub-group; in which case $p_1 = 2$, $p_2 = 3$. Thirdly, it is shown, with certain limitations, that if no prime entering in the order of a group, except the greatest, appears to a higher power than the second, the group cannot be simple. Fourthly, it is proved that no groups whose orders are of the forms.

$$p_1^m p_2 \text{ or } p_1^m p_2^2$$

can be simple. A deduction from these results of general form, aided by the consideration of certain more particular cases, is that the only simple groups whose order is the product of five primes are the known simple groups of orders 168, 660, and 1092. Finally, it is shown that if in a group of order

$$p_1^{m_1} p_2^{m_2} \ldots p_{n-1}^{m_{n-1}} p_n^{m_n}$$

the sub-groups of order

$$p_1^{m_1}, p_2^{m_2}, \ldots p_{n-1}^{m_{n-1}}$$

are all cyclical, the group cannot be simple, and cannot contain a simple sub-group.

Entomological Society, February 20.—Prof. Raphael Meldola, F.R.S., President, in the chair.—Mr. W. M. Christy exhibited specimens of *Lycæna agestis*, caught in Sussex, last summer, which had a white edging round the black discoidal spot. He said the specimens might, perhaps, be identical with the northern form of the species known as the variety *salmacis*.—Mr. H. Goss exhibited a small collection of Lepidoptera from the South of France, made by Mr. Frank Bromilow.—Prof. Meldola invited discussion upon the address delivered by Mr. Elwes, as retiring president, on the "Geographical Distribution of Butterflies," at the last annual meeting. He remarked that he had not himself had time to consider the paper in an adequate manner, but he thought that the discussion might lead to a useful expression of opinion if the speakers would deal with the question as to how far the scheme of distribution advocated by Mr. Elwes was borne out by a comparison with other orders of insects. He was of opinion that in considering schemes of geographical distribution, the results arrived at were likely to be of greater value the wider the basis on which they rested, and he therefore suggested that the question might also be taken into consideration as to how far it was justifiable to draw conclusions from the consideration of one division or one order only. Dr. Sharp, F.R.S., remarked that geographical distribution consisted of two divisions; firstly, the facts; secondly, the generalisations and deductions that may be drawn from them. He thought that as regards insects generally our knowledge of the facts was not yet sufficient to warrant many generalisations. Still the impressions of those who have paid attention to particular groups of insects are even now of some importance, though at present based on incomplete knowledge. He thought the Rhopalocera would prove to be a somewhat exceptional group in their distribution. Notwithstanding that Australia and New Zealand are so poor in them, this was by no means the case with their Coleoptera, Australia being very rich, and its fauna of Coleoptera being very distinct. He thought that if Lepidoptera generally were well collected in Australia and New Zealand, it would be found that this order was not so poor in species as was supposed. He instanced the case of the Sandwich Islands, where it was supposed that there were very few species of Lepidoptera, and yet some 500 species, or perhaps more, had been recently found there by Mr. R. C. L. Perkins, who had been sent to investigate the islands by a committee appointed by the Royal Society and British Association.—Mr. McLachlan, F.R.S., said he was of opinion that no definite demarcation of regions existed, but that all the regions over-lapped; in any case the retention of Palæarctic and Nearctic as separate provinces was not warranted on Entomological data. He believed that at the close of the Glacial period some insects instead of going north were dispersed southwards, and that the present geographical distribution of some forms might thus be accounted for. The discussion was continued by Mr. Osbert Salvin, F.R.S., Mr. J. J. Walker, Herr Jacoby, Mr. Champion, Mr. Elwes, and Prof. Meldola.—The Rev. T. A. Marshall contributed a paper entitled "A Monograph of British Braconidæ, Part vi."—Mr. J. W. Tutt read a paper entitled "An attempt to correlate the various systems of Classification of the Lepidoptera recently proposed by various authors." In this paper he criticised the opinions recently expressed by Mr. G. F. Hampson, and Dr. T. A. Chapman, in certain papers published by them. A discussion ensued, in which Mr. Elwes, Prof. Meldola, and Mr. Tutt took part.

Zoological Society, February 19.—Sir W. H. Flower, K.C.B., F.R.S., President in the chair.—Mr. F. E Beddard, F.R.S., read a paper in which he gave a description of the brain of the Glutton (*Gulo luscus*).—A second paper by Mr. Beddard contained a description of the brain of different species of Lemurs that have died in the Society's Gardens, pointing out the range of variation to be observed in the cerebral convolutions of this order.—A communication was read from Mr. C. Davies Sherborn and Dr. F. A. Jentink, in which was given the dates of the publication of the parts of Siebold's "Fauna Japonica" and Giebel's "Allgemeine Zoologie" (first edition).—A communication was read from Dr. J. de Bedriaga, "On the Pyrenean Newt, *Molge aspera*, Dugès," dealing with the external, osteological, and larval characters of this imperfectly-known Batrachian, and giving an account of its habits.

Linnean Society, February 21.—Mr. C. B. Clarke, F.R.S., President, in the chair.—Mr. J. H. Vanstone exhibited specimens of some nearly allied Hydrozoa, namely, *Bougainvellea ramosa* and *B. musca*, and after demonstrating their structure, gave reasons for concluding that although the latter had been described as distinct by Prof. Allman, the characters relied upon were not of specific value but simply varietal.—Mr. George Brebner exhibited some lantern slides of *Glœosiphonia capillaris* and other Algæ, with accompanying descriptions, and gave an interesting account of his method of preparing slides in colours.—On behalf of Mr. J. Boerlage, the President demonstrated the chief points in a paper communicated by him on the identification of *Chionanthus Ghæri*, an obscure species figured by Gaertner at the end of the last century in his famous work "De fructibus et seminibus Plantarum," but hitherto undetermined. From the researches of Mr. Boerlage it now appeared that it was evidently referable to *Seripodendron costatum*, Kurz. This was made clear by the excellent drawings which accompanied the paper, as well as by the specimens which were exhibited.—A paper was then read by Mr. E. M. Holmes, on new marine Algæ from Japan. The author pointed out that up to the present time the known species of Algæ from that country did not exceed 300, or about half the number found in Great Britain; but that the districts around three centres only had been explored, namely Hakodadi, Tokio, and Nagasaki, notwithstanding the fact that seaweeds are largely used as food by the Japanese, and form an important article of commerce. The paper included descriptions of twenty-three new species (the structure of which was shown by means of the oxyhydrogen lantern) belonging to the genera *Cladophora*, *Codium*, *Dictyota*, *Dictyopteris*, *Polyzonias*, *Chondrus*, *Gracilaria*, *Grateloupia*, *Gymnogongrus*, *Halymeria*, *Lethershedtia*, and *Padina*.

[1] It has been pointed out to the author since the paper was communicated to the Society, that this first result is contained in a paper by Herr G. Frobenius, *Berliner Sitzungsberichte*, 1893.

CAMBRIDGE.

Philosophical Society, February 25.—Mr. R. T. Glazebrook, Treasurer, in the chair.—On binocular colour mixtures, by Dr. W. H. R. Rivers. Two methods of producing binocular colour mixture were shown—by Wheatstone's stereoscope and by a modification of Hering's method devised by Mr. E. T. Dixon. Colour mixture and rivalry were described as occurring in the after-image following binocular combination of coloured patches.—On a new parasite probably allied to Echinorhynchus, by Mr. A. E. Shipley. The specimens described came from the skin of a bird *Hemignathus procerus*, taken by Mr. Perkins in the Island of Kauai, one of the Sandwich Islands.—Notes on Pachytheca (with exhibition of specimens), by Mr. A. C. Seward. The genus *Pachytheca* from Silurian and Devonian rocks of Britain and Canada has been a subject of discussion among palæontologists ever since its discovery in 1853. Several writers have placed the fossil among Algæ, and this position has been assigned to it on the grounds of a supposed resemblance of its histological structures to that of certain recent genera. An examination of a series of microscopic sections prepared by Mr. Storrie, of Cardiff, has led the author to doubt the sufficiency of the evidence on which the comparison with any existing alga has been based, and to regard *Pachytheca* as an organism of uncertain position which might well receive attention at the hands of zoologists.

PARIS.

Academy of Sciences, March 4.—M. Marey in the chair.—The life and works of Admiral Pâris, member of the Geography and Navigation Section, by M. E. Guyou.—Axoïds of two plane lines, by M. H. Resal.—Prophylactic remedy for marsh-fevers, by M. d'Abbadie. The use of a daily fumigation of the body with sulphur is urged as a preservative against intermittent fevers in malarial districts.—On the interior pressure and the virial of the interior forces in fluids, by M. E. H. Amagat. A mathematical paper in which the variations of certain theoretical constants, deduced from the results of the experimental determinations of the properties of gases under high pressures, are discussed.—Observation of Wolf's planet BP, made at Toulouse Observatory, by M. F. Rossard.—Rectification of some arithmetical theorems, by P. Pepin.—The month of February, 1895, at Parc de Saint-Maur Observatory, by M. E. Renou. A discussion of the meteorological conditions obtaining in the neighbourhood of Paris. It is shown that a continued low temperature is very rare in February. The mean temperature of the month is given at $-4^{\circ}\cdot45$. The minimum temperature was reached on the 7th at Parc de Saint-Maur $-15^{\circ}\cdot4$, and at Châteaudun $-14^{\circ}\cdot6$, and on the 9th at Vendôme $-19^{\circ}\cdot4$. At the first-named station the earth was frozen beneath turf to a depth of 0·53 metres, and in the kitchen-garden to a depth of 0·65 metres.—Panoramic views obtained with the repeating twin-camera, by M. J. Carpentier.—Basic and acid oxides and sulphides. Zinc sulphide, by M. A. Villiers.—Calorimetric researches on dilute solutions. Sodium acetate, by M. E. Monnet. The heat of solution of sodium acetate augments with the concentration of the solution.—On hexamethylene-amine; ammonium salts; action of acids; production of primary amines, by M. Delépine. The reaction of acids on the ammonium iodides of hexamethylene-amine. $C_6H_{12}N_4.RI+6H_2O=6CH_2O+3NH_3+NRH_3I$, is given as a new method of forming primary amines. The use of bismuth potassium iodide for the isolation of these primary amines is noted.—On the composition of French and foreign oats of the 1893 crop, by M. Balland.—New considerations on the comparative anatomy of the limbs, by M. J. P. Durand (de Gros). M. Edmond Perrier followed up this paper with a discussion of the theory of the compound nature of the higher animal organisms.—On a disease of the spiny lobster, by MM. E. L. Bouvier and Georges Roché.—On the formation of the shell of molluscs, by M. Moynier de Villepoix.—On the diffusion of perfumes, by M. Jacques Passy.—Researches on the fertilising materials required by the vine, by M. A. Müntz. The following conclusions have been arrived at: (1) In all vineries the absorption of nitrogen and potash is much more considerable than that of phosphoric acid. (2) Nitrogen is absorbed in large quantity by the vine, and, contrary to widely received opinions, nitrogenous manures ought to be used; these are in other respects the most effective. (3) In the southern vineyards, nitrogen is absorbed in greater proportion than potash; in more northern regions potash is most absorbed. (4) Notwithstanding the enormous difference in yield, the southern vine requires no greater amount of nutritive materials than the vines of more temperate climates. (5) The quantity of fertilising elements used by the vine per hectolitre of wine produced is three or four times more considerable in the more northern districts than in the south.—On the abnormal partitions of the fronds of ferns, by M. Adrien Guébhard.

BOOKS, PAMPHLETS, and SERIALS RECEIVED

BOOKS.—Abrégé de la Théorie des Fonctions Elliptiques: C. Henry (Paris, Nony).—Mechanics—Statics: R. T. Glazebrook (Cambridge University Press).—Steam-Power and Mill-Work: G. W. Sutcliffe (Whittaker).—Scientific and Technical Papers of Werner von Siemens. Vol. 2, Technical Papers, translated (Murray).—From a New England Hillside: W. Potts (Macmillan).—Prince Henry the Navigator: C. R. Beazley (Putnam).—Catalogue of the Birds of Prey, with the Number of Specimens in Norwich Museum: J. H. Gurney (Porter).—Annuario publicado pelo Observatorio do Rio de Janeiro, 1894 (Rio de Janeiro).—A Theoretical and Practical Treatise on the Manufacture of Sulphuric Acid and Alkali: Dr. G. Lunge, 2nd edition, Vol. 2 (Gurney and Jackson).—A Students' Text-Book of Botany: Dr. S. H. Vines (Sonnenschein).—The Origins of Invention: Dr. O. T. Mason (Scott).—Die Gesellschaftsordnung und ihre Natürlichen Grundlagen: O. Ammon (Jena, Fischer).—Die Grundgebilde der Ebenen Geometrie: Dr. V. Eberhard, 1 Band (Leipzig, Teubner).—Vorlesungen aus der Analytischen Geometrie der Kegelschnitte: F. Dingeldey (Leipzig, Teubner).—The Astrologer's Ready Reckoner: C. J. Barker (Halifax, Occult Book Company).—The Voyage of H.M.S. *Challenger*. A Summary of the Scientific Results (with Appendices), 2 Parts (Eyre and Spottiswoode).—Le Climat de la Belgique en 1894 (Bruxelles).

PAMPHLETS.—Sweet Cassava: H. W. Wiley (Washington).—Neudrucke von Schriften und Karten über Meteorologie und Erdmagnetismus, No. 4 (Berlin, Asher).—Über die Grundlagen und Ziele der Raumlehre: Dr. V. Eberhard (Leipzig, Teubner).—A Summary of Progress in Mineralogy and Petrography in 1894 (Waterville, Me.).—Sociedad Cientifica Argentina. Flores é Insectos: A. Galllardo (Buenos Aires).—The Basic Massive Rocks of the Lake Superior Region: W. S. Bayley (Chicago).

SERIALS.—Geographical Magazine, March (Stanford).—L'Anthropologie, tome vi. No. 1 (Paris).—Bulletin of the American Mathematical Society, February (New York).—Journal of the Chemical Society, March (Gurney and Jackson).—Natural History of Plants, Part xi. (Blackie).—Verhandlungen der K.K. geologischen Reichsanstalt, Jahrg. 1894, No 1 bis 18 (Wien).—Insect Life, vol. 7, Nos. 1–5 (Washington).—American Journal of Science, March (New Haven).—Bulletins de la Société d'Anthropologie de Paris, No. 8, 1894 (Paris).—Psychological Review, March (Macmillan).

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